Assessing the Demographic Consequences of Major Development Projects

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Assessing the Demographic Consequences of Major Development Projects

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NOTE

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In some tables, the designations "developed" and "developing" economies are intended for statistical convenience and do not necessarily express a judgement about the stage reached by a particular country or area in the development process.

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PREFACE

The United Nations has always viewed population issues in the context of achieving overall improvement in the social and economic conditions of the population. The General Assembly and the Economic and Social council have consistently called for comprehensive understanding of the nature of linkages between population and socio-economic development. The World Population Conference held at Bucharest in 1974, adopted specific resolutions for integrating demographic factors into development planning; a decade later, these resolutions were reaffirmed at the International Conference on Population held at Mexico City.

In response to the resolutions of both conferences, a great deal of research was initiated, studying the nature of the relationship between population and socio-economic factors. However, major emphasis has been placed on only one aspect of integration: adapting development plans and programmes to take into account projected levels and trends in population factors. The other aspect of the integration process, namely, taking into account the effects of development plans and programmes on demographic patterns and trends, has received much less attention. Very few planning exercises have included studies designed to assess the demographic consequences of specific development plans and projects.

In 1979, the United Nations Fund for Population Activities (UNFPA) and the Rockefeller Foundation jointly organized a workshop which directed the attention of the international development community to the importance of research in this less studied area by demonstrating that development projects had significant impacts on demographic factors. Following this initial activity, several institutions including the Population Council, the International Development Research Centre (IDRC), the University of Michigan, the United States Agency for International Development (USAID), the International Labour Organisation (ILO) and the World Bank, undertook research in this area.

The Population Commission of the United Nations at its twenty-third session recommended that the Population Division of the Department of International Economic and Social Affairs of the United Nations Secretariat should also carry out research to assess the demographic consequences of major development projects. In 1985, therefore, with the support of UNFPA, the Population Division initiated a project whose objective was to develop a cost-effective methodology for investigating the nature and magnitude of such impacts of demographic factors. To assess progress and propose guidelines for future research, a workshop was held in connection with the project. This volume presents the report and recommendations of the Workshop on Assessing the Demographic Consequences of Major Development Projects, as well as selected papers that were presented and discussed.

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Part Two. Background papers

INTRODUCTION

The importance of integrating population variables into development planning has been increasingly recognized since the convening of the World Population Conference at Bucharest in 1974. It was given particular relevance at the International Conference on Population held at Mexico City in 1984, which recommended that "national development plans and programmes, as well as international development strategies should be formulated on the basis of an integrated approach that takes into account the interrelations between population, resources, environment and development." 1/

In line with this increased recognition a substantial literature has evolved on the interrelationship between population and socio-economic factors in the process of development. In actual planning practice, however, population factors are in most cases only passively integrated into development plans; that is, plans are drawn to respond to projected levels and trends in population factors, or to accommodate them, not actively to seek specific demographic outcomes. Population variables are considered exogenous, rather than endogenous to the system.

The situation described has been due, at least in part, to the relatively slow development of methodologies applicable to the conditions of limited data and scarce human resources prevailing in most developing countries. This is particularly the case at the micro level. While considerable work has been devoted to understanding behavioural relationships and interactions between demographic and socio-economic processes (up to and including the formulation of complex macro-economic models), analysis of the demographic impact of specific development projects has lagged behind.

Development planning projects have generally been selected for their expected socio-economic effects, and possible demographic effects have not been much considered. The projects undertaken could, and sometimes did, lead to demographic changes for which no provision had been made. An opportunity was missed to incorporate into the projects features that would increase desired demographic impacts or reduce those considered negative. Finally, demographic impact was not often utilized as an additional factor in the cost-benefit calculations to improve project selection at the margin.

More recently, researchers have given greater attention to the subject of project impact studies in the late 1970s and 1980s. For example, in 1978, the Population Council initiated a programme on the impacts of fertility on development in Asia; in 1979, a World Bank workshop developed criteria to determine which of the Bank's development projects were amenable to the study of demographic impacts; also in 1979 and 1981, the United Nations Fund for Population Activities (UNFPA) and the Rockefeller Foundation sponsored two workshops of the International Consortium of Research Centers on Population

and Development, the end result of which was a publication, in 1983, of a monograph on the impact of rural development projects on population; 2/ in 1982, the Population Division of the Department of International Economic and Social Affairs of the United Nations Secretariat commissioned a background review of the problems, entitled "Assessing the demographic impact of non-demographic development projects", 3/ which contained suggestions for further research; in the same year, the University of Michigan published case studies on the subject; 4/ and in 1983, a report was prepared for the Government of Pakistan on a framework for monitoring the impact of development projects on fertility. 5/

Among other conclusions, these studies demonstrated the importance of taking into account the cultural and historical context in which projects are implemented. They also pointed to the need to develop methodologies that would reduce the requirements for data beyond those available from censuses and other traditional sources.

The project

A project entitled "Assessing the demographic consequences of major development projects" is being implemented by the Population Division with funding from UNFPA. Work started in January 1985, and it is expected to be completed in December 1987. In designing the project, account was taken of the findings of earlier studies refined on the basis of the experience of several specific studies.

The immediate objective of the project is to contribute to the development of a practical methodology for assessing the demographic impact of development projects that will be applicable in different countries and to a broad variety of project situations. It is meant to meet more general objectives, by facilitating the consideration in development planning of the impact of major projects on fertility, mortality and migration, and by promoting the design of such projects in a way consistent with demographic objectives on a national scale.

The approach developed is based on available sources of routinely collected data, such as censuses and vital registration systems. In cases where this information is not fully available, the methodology makes use of appropriate indirect means of demographic estimation.

By studying development projects representative of those implemented in many developing countries, which extend over a range of cultural and historical contexts and geographical regions, the results should be widely applicable. Projects in the following countries were selected: Cameroon, Costa Rica, India, Morocco and Pakistan. In each country, a leading national research institution collaborates with the Population Division, and a consultant from each has been designated to undertake the country study. The institutions involved are: Centre de recherches économiques et démographiques (CRED), Cameroon; Instituto de Investigaciones en Ciencias Económicas, Costa Rica; International Institute for Population Sciences (IIPS), India; Centre d'études et de recherches démographiques (CERED), Morocco; Pakistan Institute of Development Economics (PIDE), Pakistan.

In order to promote and advance consideration of the demographic impact of projects by planners, individual case studies and a comparative study, carried out with the underlying methodology, will be published separately and disseminated to planners, to training programmes in population/development planning and to relevant organizations.

Considering that the rural population is the majority in most developing countries, rural development projects have been selected. All but one of them 6/ are concerned with electrification or irrigation because these infrastructures and services generally involve large interventions that have direct effects on a number of socio-economic variables influencing the standard of living of numerous people, and it is in response to such direct effects that demographic changes can be expected to occur.

To promote an exchange of ideas and experiences among all researchers involved a workshop, with funding from UNFPA, was planned to take place at an appropriate stage in the research process. The Workshop was to include presentations on methodological issues by experts in the field, and to relate the experiences of other researchers in similar recent undertakings. The views and concerns of planners were also to be heard, as the ultimate purpose of the methodology to be developed was to provide a tool to enable them to take into account demographic impact in designing and selecting projects.

The country consultants' first task was the preparation of histories of the selected projects and inventories of relevant available data. On the basis of these, outlines were developed for discussion at the Workshop.

The Workshop was, thus, the culmination of the preliminary activities. The discussion concerned issues raised by the preliminary methodological guidelines and the preliminary case study report. Recommendations were formulated for the last phase of the project, in which the case studies and methodology were to be completed, and recommendations made for future research on analysis of the demographic impact of projects.

The present volume is divided into two parts. Part one contains the report and recommendations of the Workshop. Part two comprises the background paper prepared by the Population Division and thematic papers commissioned for the Workshop. They are grouped according to the following topics: (a) general overview; (b) experiences of other institutions with population impact analysis; and (c) methodological and measurement issues in assessing the demographic consequences of major development projects.

Notes

1/ United Nations, Report of the International Conference on Population, Mexico City, 6-14 August 1984 (United Nations publication, Sales No. E.84.XIII.8 and corrigenda), chap. III, sect. A, recommendation 1.

2/ Richard E. Bilsborrow and Pamela DeLargy eds., Impact of Rural Development Projects on Demographic Behaviour, Policy Development Studies, No. 9 (New York, United Nations Fund for Population Activities, 1985).

3/ Warren C. Robinson and Sarah F. Harbison, Assessing the Demographic Impact of Non-Demographic Development Projects (State College, Pennsylvania, Economic Research Associate, 1982).

4/ Robin Barlow, ed., Case-studies in the Demographic Impact of Asian Development Projects (Ann-Arbor, University of Michigan, 1982).

5/ John F. Kantner and Warren C. Robinson, "A framework for monitoring the impact of development projects on fertility" (Baltimore, Johns Hopkins University, 1983).

6/ In Cameroon, the demographic effects of rural co-operatives are being studied.

Part one

REPORT OF THE UNITED NATIONS WORKSHOP ON ASSESSING THE DEMOGRAPHIC CONSEQUENCES OF MAJOR DEVELOPMENT PROJECTS

I. OPENING OF THE WORKSHOP

The Workshop took place in New York, from 1 to 4 December 1986, with the participation of eight experts, two planners from developing countries, eight representatives of non-governmental and international organizations, the project expert, five country consultants, and members of the Population Division and UNFPA. A list of the participants is given in annex I below. The papers contributed were presented in four groups: (a) an overview, consisting of the preliminary methodological approach and data set proposals prepared by the Population Division, and papers on the application of population impact analysis to development planning in Mexico and Cuba; (b) experiences with population impact analysis by the Population Council, the Carolina Population Center, the International Development Research Centre and the International Labour Organisation; (c) reviews of the five-country studies in progress; and (d) papers on specific methodological issues as follows: the problems of data and measurement in demographic impact assessment based on census data (David Sly); conceptual issues and possible analytical strategies for the project, given the proposed data matrix (Albert I. Hermalin and Krishnan Namboodiri); and methods of indirect demographic estimation for small areas (Kenneth Hill). Annex II to the report gives a list of the documents considered. The agenda of the Workshop appears in annex III.

The Director of the Population Division, Jean-Claude Chasteland, opened the Workshop. He remarked that the international conferences of 1974, at Bucharest, and 1984, at Mexico City, had stressed the importance of integrating population variables into development planning, but did not provide a clear concept of integration. Therefore, he said, emphasis had since been placed on the better understood aspect of development planning, i.e., response to demographic change, while little emphasis had been given to the other aspect, that which took into account the effect of development plans and programmes and, in particular, projects on population. The Population Division's project on assessing the demographic consequences of major development projects was a contribution to research on the second, less understood, aspect.

Mr. Chasteland explained why the project sought to evaluate the extent to which existing censuses and other routinely collected data could be utilized for impact analysis, rather than undertaking special-purpose household surveys. That, he said, meant that the geographical areas for which censuses collected data would be the project's units of analysis. He further noted that the demographic impact of development projects reflected the demographic behaviour of households, and that there were community level effects as well.

Mr. Chasteland also discussed the Workshop's agenda and commented on the timeliness of the research endeavour to be discussed, which has been confirmed by the readiness of leading national research institutions in five countries to collaborate in it and to finance a substantial portion of the necessary expenses. He concluded by thanking all the participants for their spirit of

co-operation and by expressing his confidence that the recommendations of the group would be useful for further research and for finalizing the country case studies.

The deliberations began with reference to the four major objectives of the workshop: to provide guidance to the Population Division on the steps necessary to finalize the project; to the funding agencies on promising directions for moving forward in this area of research; and to researchers and planners. The recommendations of the Workshop would address those objectives. Therefore, the group would consider methodological issues in the broadest possible terms, giving adequate weight to theoretical aspects, as well as to practical experiences of the participants engaged in research in the area. Although there was a need for concrete recommendations to guide the country consultants in completing the case studies, the practical concerns of planners would also be borne in mind.

In their deliberations, the participants discussed conceptual issues that were basic for the determination of the appropriate research design. Questions were raised about the time frame of the study, about the level of analysis and other methodological issues. The implications of the questions raised and issues debated for the country studies and for researchers in general were considered with particular reference to the stated or inferred needs of planners.

In the present report, the discussions are organized according to the following outline: (a) methodological considerations; (b) implications for planners; and (c) recommendations.

II. METHODOLOGICAL CONSIDERATIONS

A. Conceptual framework

The following conceptual framework constitutes the basis of analysis for the project "Assessing the demographic consequences of major development projects": a causal linkage from a development project (or its input) to a demographic response operates through changes in social and economic factors. In assessing the fertility consequences of development projects, the conceptual framework suggests that a development project, by changing social and economic factors, leads to changes in the proximate determinants that in turn lead to changes in fertility. However, a fully articulated theory of how demographic change is brought about by a development project would include specification of: (a) the intermediate variables through which changes in the socio-economic factors affect either migration or mortality; (b) the time frame within which a development project affects the intermediate variables and the time for those changes to influence the demographic variables; and (c) the interrelationships among the demographic variables themselves, which add another dimension to the final demographic outcome.

The participants identified two types of problems in measuring the strength of the relationship between a development project input and its demographic effect: first, those encountered in measuring the observed relationship for a given development project; and second, those encountered in assessing the relative effects of different projects.

For each individual project, in the case of fertility for example, the theoretical model suggested a causal linkage running from the project input to socio-economic factors, to changes in proximate determinants, to fertility. Using individual level data, a small correlation coefficient might be found corresponding to each of the three linkages, which, when multiplied, would give a very small total effect. However, it was pointed out by a participant that more of the variance could be explained with community-level data. But it was stressed that it was not the size of the effect or the level of the coefficient that was most important, but rather a better understanding of how the process took place. That underscored the importance of specifying explicitly the theory linking development projects to demographic outcomes.

Moreover, the participants agreed that the methodological approach to assessing the demographic consequences of individual projects would differ for each project, depending on its objective. A different research strategy was needed to assess projects aimed at raising the level of income than to assess those designed to improve specific aspects of welfare, such as health or education.

With respect to the relative effects of different projects, comparability between the results of different studies should be sought by standardization of the conceptual framework, the study designs, model specifications and measurements. It might be tempting to use ecological analysis, which might give higher correlation coefficients, but that type of analysis might also lead to spurious conclusions. Thus, it was suggested that it would be advisable to compare changes at both the macro- and at the micro-levels. One should first understand the mechanisms through which macro- or micro-level factors affected demographic behaviour. Ecological analysis could then be used to identify which of those factors explained demographic differences among communities. In addition to being based on an appropriate theoretical approach, that would permit drawing valid policy recommendations.

Unfortunately, it was observed, that approach imposed very heavy requirements for data. Nevertheless, the usual challenge was not to get as much information as possible, but to reduce the amount of information required, which could only be done on the basis of a theory.

The importance of the theoretical basis having thus been re-stated, it was pointed out that household behaviour - which determined demographic outcomes - adapted to perceived circumstances in varied and unanticipated ways. Therefore, it was concluded that a single theoretical model might be inadequate to explain the mechanisms of each of the demographic responses.

B. Research design

The participants noted that the selection of a specific development project for study implied a particular geographic area and a specific time-frame. Studies that assessed effects at the national level, due to their high level of aggregation tended to mask important regional differentials. The project studies, with their narrower focus, could provide policy inputs for planners at sub-national, regional levels.

The electrification and irrigation projects selected for the country studies, while focused on specific areas, affected large numbers of people. It was agreed, however, that for the purposes of the study the definition of areas as "electrified" or "irrigated" must be precise and the extent of use of electricity or irrigation must be ascertained. It was pointed out, for example, that in India there was a tendency to define a village as being electrified as soon as a line had reached it, even if very few households had tapped into it. Data collected on the basis of that definition could lead to mistaken conclusions.

In terms of time-frame, the participants agreed that the length of the time-frame built into a research design should depend on the time-lag between the initiation of a development project and the expected occurrence of the demographic effect. But, it was noted, there was no theory on the basis of which one could predict either the time it would take for a project input to produce changes in the socio-economic conditions, or the time required for changes in those conditions to bring about a demographic change of a given size.

If too short a time-frame was selected, the demographic effect under study might not yet be manifest. If the time-frame was too long, the experiment might be contaminated. Contamination might be due to spill-over of development inputs into the control areas - if these were adjacent to the experimental areas - or to other exogenous factors occurring over the longer term. There was also a dearth of knowledge on the manner in which the relationship between development inputs and demographic variables changed over time.

There was consensus that the ideal methodological approach should involve an experimental/control design and a longitudinal time-frame. It was pointed out that an ideal application of this approach required that: (a) the project should be implemented in a large, randomly selected number of area units; (b) there should be no other concurrent development projects in the areas; (c) information should be collected before and after introduction of the treatment (electrification or irrigation).

If requirement (a) was met, areas not receiving the project might be compared with those receiving it and could serve as controls even if requirement (b) was not met, as long as the other projects were randomly distributed and their effects on the criterion variable were smaller than those of the project under investigation. But requirement (c) added a time dimension that might lead to contamination of the experiment, that is, to changes brought about by other development interventions. Isolating the demographic effects of the project under study then became very difficult.

The participants raised and discussed the possibility of substituting a cross-sectional perspective for a longitudinal one, as it was claimed that cross-sectional differences reflected long-run conditions. They agreed that the comparability between the experimental and the control areas, studied cross-sectionally, would be difficult to establish. Nonetheless, given the data and resource constraints generally found in third world countries, the research strategy might have to rely on analysis based on cross-sectional differences.

The presentation of previous experiences with impact studies highlighted some important lessons with respect to research design. For example, the experience of the International Development Research Centre included one study, undertaken in the Philippines, with a framework for analysis that facilitated the explanation of socio-economic and demographic changes that would otherwise be unexplainable. 1/ That framework combined (a) an experimental design with micro-level observations over a long period of time; (b) the utilization of varied sources of data; and (c) the consideration of qualitative, contextual factors. In the case of the study of the Comilla Programme in Bangladesh, 2/ it was shown that given the small size of the sample and in the absence of longitudinal data or an experimental design, it could not be established how much of the demographic effect found was due to the programme and how much would have occurred without it, as a result of overall modernization or other societal changes.

The participants agreed that, given the near impossibility of finding areas that fit the requirements of an experimental design with a longitudinal perspective, the researcher should interpret the results on the basis of knowledge of the historical context of the area where the project was located, as well as a qualitative understanding of the process that related a development input to a demographic change.

C. Threats to validity

It was pointed out that the validity of the results obtained by applying an experimental design could be questioned on several grounds. First, if certain units were chosen to receive the development intervention because the others, or controls, already had substitutes for the inputs provided by the development project (that is, the controls did not need them, or they already had them, or the treatment areas were being compensated for not having the project), there was no basis for making a comparison. Second, validity requirements were not met if the experimental units were more predisposed to receive the project inputs than the control units; then, one must be concerned with preselectivity, i.e., the process by which development inputs were allocated. In so far as those inputs were determined by political or ecological factors, inferences about the effects of the inputs might be misleading. For that reason it was important to determine how developmental projects were allocated to specific areas. Third, the people living in close proximity to a project might be indirectly influenced in terms of their predisposition to change; in such cases, the results might also be questioned.

D. Level of analysis

In considering the choice between a macro- and a micro-level approach, the participants suggested that the level of analysis should depend on the purpose of the study. If the purpose was to examine the household's demographic response to a development input, a micro-level approach was appropriate. If interest lay on differentiating communities in order to determine which institutional and community settings were consistently and reliably related to a demographic outcome, a macro-level approach was called for. If the goal was to understand the mechanisms and circumstances through which a macro-level intervention could affect micro-level demographic behaviour, then a multi-level approach should be adopted.

It was pointed out that if the multi-level approach was adopted, a larger proportion of the variance could be explained than if the study was conducted at only one level. Furthermore, that approach offered a greater potential for the derivation of sound policy recommendations.

The group observed that the experience of the Population Council, the International Development Research Centre, the Carolina Population Center of the University of North Carolina and the International Labour Organisation also provided valuable lessons with respect to level of analysis. For example, the Population Council presented the results of one study to illustrate the effect of data problems on inferences drawn from ecological studies. Using villages as the units of analysis, a multiple regression analysis demonstrated a significant negative relationship between birth rate and the level of availability to the village of irrigation, electrification and health station. The study, however, cautioned that such conclusions would be highly suspect because village populations were not large enough to produce robust fertility estimates and because further analysis did not justify an assumption that errors were randomly distributed. In general, the results of such analyses turned out to be unreliable because: (a) when village-level data were obtained by aggregating household data, the number of households sampled per village was too small; (b) the small number of villages in the sample resulted in insufficient observations in certain cells; (c) where the number of villages was adequate, the small size of their population reduced the reliability of estimates of village birth rates; and (d) village birth rates were also affected by under-registration, which was a chronic problem in most developing countries.

It was mentioned that some of the problems associated with ecological analysis - i.e., poor and unreliable data - could be minimized by increasing the sample size. But the deficiencies of vital registration systems could not be resolved easily. One solution would be the complete enumeration of the population of all villages included in the study.

The participants were informed of the experience of the University of North Carolina in Ecuador, where detailed demographic data were being obtained at community-level, thus eliminating the need for surveys. The group suggested that the approach might not be applicable elsewhere because definitions of communities and their boundaries might change over time and in some low

density countries it might not be possible to determine boundaries (that, it was noted, was the case in Cameroon), and because the reliability of retrospective data obtained through interviews of community leaders had not been established. The latter issue would be explicitly examined in Ecuador.

The group concluded that under ideal conditions the level of analysis would be determined by the nature of the research questions being pursued. Under actual conditions, however, it was likely to be constrained by the availability and reliability of data.

E. Data and measurement

The participants observed that data collection being generally more expensive than tabulation, reliance on census and other institutionalized sources of data could be a useful strategy; and since census data were readily available for geographical area units, areal analysis was an appropriate approach for the study of demographic impacts. They pointed out, however, that there were important limitations in using census data. For example:

(a) The data must be available at the appropriate level of disaggregation by area. Additional specialized data processing would often be necessary, requiring the assistance and collaboration of the government census or statistical office;

(b) Most countries counted people de facto - where they were at the time of the census - rather than at the usual place of residence. This might result in over-enumeration for places that attracted large numbers of visitors or temporary residents, and under-enumeration for places from which the visitors had departed. Such a situation, in turn, affected the calculation of population change between two censuses and thus the calculation of vital rates. It was also pointed out that, since family members might be enumerated in different places, it was difficult to obtain family statistics from de facto counts and thus to calculate rates of migration. In addition, the time of the year and even the day of the count would influence the reported size of local area populations based on de facto enumeration;

(c) Census areas were generally based on political and administrative divisions that might not coincide with the geographical area of the development project. It would then be necessary to transform the political or administrative units used in the census into the project areas by interpolation;

(d) If more than one census was used, definitions of areas might have changed. If so, the data must be adjusted so the units utilized for the analysis were constant over time.

The participants also noted that in the short term, it was more likely that changes would be observed in migration, and perhaps mortality, than in fertility. Thus, in the case of project studies built around two censuses that emphasized fertility, it was possible that the time-lag necessary for a fertility response would be longer than that of the proposed data matrix.

They noted that in a study in progress in Ecuador, the value of census data had been limited because the duration of the projects was shorter than the intercensal time-interval. Since most censuses were taken 10 years apart, a participant suggested that census data were useful only if a project had been in existence for more than 10 years. Under ideal conditions, the initial census would correspond to the beginning of the project, thus providing a base-line.

Commenting on other data sources, the participants noted that:

(a) Although the coverage of civil registries was very incomplete in most developing countries, even a minimal coverage was useful in complementing data from other sources. It was less so, however, if tabulations were by place of occurrence rather than by place of residence;

(b) The data base could be enriched by sample surveys. Ideal for capturing the demographic effects of a development project would be a baseline survey followed by a multi-round one. The cost would be, however, very high. A single-round retrospective survey might thus be considered sufficient;

(c) Community-level surveys in rural areas such as villages, might provide a useful alternative to expensive household sample surveys and to census and registration data that might have low reliability. However, their value for determining the demographic impacts of development projects (or other purposes relating to development) remained to be assessed. Experiments with their use were highly desirable.

In turning their attention to the question of measurement, the participants acknowledged that research could focus on the influence of the development project on any of the following four aspects: (a) the rates at which demographic processes were changing; (b) the manner in which overall population growth was affected, including the relative contribution of each growth component; (c) the manner in which population composition was affected or (d) changes in population composition resulting from demographic changes occurring differentially to different population groups (by age, sex, ethnicity etc.).

The data set that was to be prepared for the current studies was divided into the following categories: (a) irrigation or electrification inputs; (b) economic and social changes such as increases in income or improvements in health indicators; and (c) changes in demographic characteristics, i.e., fertility, mortality and migration. The information was to relate to more than one point in time for both experimental and control groups. Given the availability of time series data, the analysis was to be based on (a) plotting of the observations over time to explore for patterns of change; (b) viewing each unit as its own control; or (c) pooling of cross-sectional and time series data.

In discussing the need for census tabulations for areal units, or areas corresponding to the development project under study, the participants

observed that those areas should be sufficiently large in terms of population to provide an adequate number of observations. The definition of "small area", it was observed, might vary from one situation to another. But for cases such as those in the present project, a population of 10,000 was proposed by one of the participants as the minimum necessary to permit the application of available methods of indirect demographic estimation to census data with confidence that the estimates would be robust. In that regard, it was noted that if the project area was quite small, it would be very difficult to find units that met validity requirements.

The participants also observed that indirect estimation techniques for demographic parameters generally assumed a closed population and sometimes even a stable one. Population of small areas, particularly those affected by major development projects, were unlikely to meet that assumption.

Two possible approaches were suggested in this context. The first was to use existing methods, but to apply them to populations that could be regarded as approximately closed to migration. Use of that approach would often provide reasonable estimates of fertility and child mortality. The second approach, designed to handle open populations, would be essential for the study of migration and was generally preferable for the study of adult mortality. A series of consistently defined data sets, including at least two census age distributions, was required for the latter approach; specific information about migration would be useful, but not required.

III. IMPLICATIONS FOR PLANNERS

The participants observed that in many developing countries, migration tended to be, at the present time, the demographic variable of most immediate concern to planners and policy makers in assessing the effects of development projects. This was partly because the effects of development projects on migration were likely to occur in a shorter time than on the other demographic variables, thus facilitating the consideration of migration effects in medium-term development plans and programmes. Planners might also be more concerned with the implications for migration than for fertility because of the immediate relationship of migration to labour distribution and employment. In Mexico, for example, the Government has for some time invested in a programme of new towns and industrial parks which aimed at colonization and industrialization of new areas in order to relieve the demographic pressure from urban areas, especially the Mexico City metropolitan area. It was important at present, it was said, to investigate the effects of the programme on labour mobility and employment.

On the other hand, the group was informed that planners in some countries might not be as interested in migration as those in others; first, because there were many different types of migration, and second, because it was not sufficiently clear which were the important questions on migration in the context of development projects. Also, some were generally more concerned about problems due to high fertility or mortality.

Independently of the demographic variable focused upon, the level of analysis also had direct implications for planners and policy makers. Their analyses and decisions were made at the aggregate level, while behaviour, whether economic or demographic, was individual.

It was imperative to understand the process through which a demographic change was caused by a development input in order to avoid misleading policy conclusions. For example, demographic impact analysis might indicate that a decline in fertility was associated with the provision of electricity, but fail to describe how the decline occurred. Knowledge of the process might indicate that there was a need to use electricity to promote the education of young women in order to secure the demographic result. Without such understanding, the policy-makers would not be in a position either to draw correct conclusions relevant to policy, or to implement them.

It was suggested that aggregate-level studies could be useful in identifying which development projects should be studied more in depth. Demographic impact studies would establish which components of the development intervention would have the greatest demographic impact, and thus be useful for improving project design. For planners then, the usefulness of impact studies did not lie only in investment allocation or priority ranking of development projects - which were primarily based on socio-economic benefit and cost considerations - but in the improvement of project design as well.

IV. RECOMMENDATIONS

The experts emphatically reiterated the timeliness and the importance of research efforts in assessing the demographic consequences of major development projects. Following intensive discussions of the theoretical and methodological issues involved and the implications of those issues in undertaking empirical research, the expert group recommended a variety of actions which, in its view, would contribute to a better understanding of how development projects affected demographic behaviour. The expert group noted that the proposed recommendations addressed the long-term goal of developing a methodology for assessing, monitoring and projecting the likely demographic consequences of major development projects. That would contribute, the experts agreed, towards the integration of population variables into development planning.

A. General

(1) In view of the limited knowledge of the effects of major development projects on demographic variables, and given the need to fill this gap, the proceedings together with revised versions of the papers presented, should be published. Efforts should also be made by the Secretariat within the limits of available resources to pursue the subject by means of further studies.

(2) Numerous studies on the demographic impact of development interventions have been undertaken and are under way, but not all are or will be published.

An inventory of all those studies should be undertaken, particularly ongoing studies in developing countries. The International Population Information Network (POPIN) may be an appropriate institution to carry out this function.

B. Country studies

(3) The following questions should be answered in the research design for all projects selected:

(a) In deciding to undertake the specific project: (i) What were the objectives of the planners? (ii) What economic and social variables did they take into account?

(b) What economic and demographic changes did actually occur?

(c) In selecting the methodology to assess these changes, how should the planning decisions be taken into account when considering the relevant factors that might affect demographic behaviour?

(4) In deciding on a particular development project and its location, the method of area selection is likely to be related to the demographic and intermediate variables of interest and their trends. For this reason, it should be determined how development projects were allocated to specific areas and this information should be used to the extent possible in the selection of control areas.

(5) In designing demographic surveys in the future, it will be useful to stratify by the presence or absence of various development projects, if there is an interest in assessing the effects of those projects. In this way, a considerable quantity of household and individual data can be obtained at no additional cost, about those who have or have not benefited from the development input. These data can then be combined with information collected about the project and the community to form the basis of a multi-level analysis.

(6) Techniques for indirect estimation of demographic parameters generally assume a closed population and sometimes a stable one. Populations of small areas, such as those affected by major development projects, are unlikely to meet these assumptions. Wherever possible, existing methods should be applied to populations that can be regarded as approximately closed to migration. When that is not possible, flexible methods designed to handle open populations should be further developed and used.

C. Comparative studies

(7) Comparative analysis should be designed to produce guidelines for assigning or altering the priorities of development projects on the basis of their demographic effects and for improving project design so as to enhance the desired demographic impact. This should involve close and active collaboration between researchers and planners and decision-makers.

D. Further research

(8) Infrastructure projects are often supportive of broader development programmes. It should be decided whether the impact of the infrastructure project itself or of the broader agricultural or industrial development project or programme is the proper focus of study.

(9) Researchers working on demographic impact should first develop their conceptual framework and indicate why and how they expect a particular development project to influence demographic behaviour; then determine whether data are available to test those hypotheses. They should seek to quantitatively assess the impact on at least intermediate socio-economic variables, as evidence suggests that if the impact is small, demographic effects are unlikely to be observed.

(10) Impact analysis should strive to follow an experimental approach, such that: (a) the analysis may be carried out over a large, randomly selected number of areas; (b) there will be no other concurrent development projects in the areas; and (c) both baseline and subsequent instruments may be used to collect detailed information on households affected to compare them "before" and "after".

(11) The utility of impact studies would be increased by monitoring the changes associated with development projects from their beginning. Thus, the decision to conduct an impact study should be taken at the start of a project, and plans should be made for two comparable household surveys in each project and control area, the first in each case being the baseline survey. Small-scale, inexpensive pilot studies might precede these in order to generate and refine hypothesis.

(12) A longitudinal or an experimental design combined with qualitative information over the life of the project, including anthropological methods, can yield useful quantitative data, permit the understanding of contextual influences, and help to interpret unexpected findings. Thus, in order to draw more valid conclusions and to make these more understandable to decision-makers and thus more useful for project planning and design, a combination of methodological tools should generally be used.

(13) Impact studies should give attention to the issue of time-lag between the developmental inputs and the demographic effects. A priori judgements are needed to assist in the design of experiments, but empirical testing of various time lags is essential, as well. For example, colonization/settlement and other projects that alter income-earning prospects are likely to have significant effects on migration in the short run, but on fertility and mortality only in the long term. Such lags should be carefully considered in designing surveys to collect information for the purpose of demographic impact assessment.

(14) There can be no single way of evaluating the demographic impact of the great variety of development projects that are undertaken. Research efforts should be directed not so much to the construction of a generalized

model as to determination of the advantages and disadvantages of different methodologies and combinations thereof for specific cases of projects, demographic variable and/or context.

(15) Impact studies should be designed to facilitate the formulation of packages of policy interventions that take into account the demographic impact of the constituent projects and/or make provision for enhancing the desired demographic effects.

E. Applications to planning

(16) More studies should be undertaken dealing with integrated projects that have multiple inputs, particularly those that include family planning as well as development inputs. Although the impacts of such projects are more difficult to evaluate or to separate out, in project implementation there are often synergistic effects which, properly planned, can be used to advantage.

(17) The emphasis in impact assessment studies should be an investigation of the immediate impact of project interventions on intermediate socio-economic and demographic variables, which may be more easily monitored than levels of fertility and mortality. Changes in these intermediate demographic variables (e.g., age at marriage, contraceptive acceptance, period of lactation) can be measured even before these changes become manifest in long-term demographic change.

Notes

1/ Carol Vlassoff, "Assessing the demographic impact of development projects: the IDRC experience". See part two of this volume.

2/ Ibid.

Annex I

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Annex II

LIST OF DOCUMENTS

Symbol	Agenda item	Title
IESA/P/AC.21/1	-	Provisional agenda
IESA/P/AC.21/2	2	Assessing the demographic consequences of electrification and irrigation projects using areal analysis (United Nations Secretariat)
IESA/P/AC.21/3	2	Population impact of urban and regional plans in Mexico (Gustavo Garza)
IESA/P/AC.21/4	2	Project on assessing the demographic consequences of major development projects: concerns of planners in Cuba with regard to integrating demographic consequences into development planning at the project level (Eramis Bueno Sanchez)
IESA/P/AC.21/5	3	Impact of development projects on fertility in Asia (A. Jain and J. Stoeckel)
IESA/P/AC.21/6	3	Methodological issues in assessing the population impact of rural development projects: lessons from the international consortium and an on-going community level data collection project in Ecuador (R. E. Bilsborrow)
IESA/P/AC.21/7	3	Assessing the demographic impact of development projects: the IDRC experience (Carol Vlassoff)
IESA/P/AC.21/8	3	Demographic impact of development projects: a brief note on ILO's country review studies (A. S. Oberai)

IESA/P/AC.21/9	4	Demographic consequences of a development project: Maharashtra State, India (S. Mukerji, S. Kulkarni and N. Audinarayana)
IESA/P/AC.21/10	4	Demographic consequences of irrigation development in Punjab province of Pakistan (G. Y. Soomro)
IESA/P/AC.21/11	4	The impact of rural electrification on certain demographic variables in Costa Rica (J. D. Trejos-Solorzano)
IESA/P/AC.21/12	4	A brief description of the selected irrigation project (Abdellatif L'Farakh)
IESA/P/AC.21/13	4	Demographic consequences of a development project: a study of the co-operative movement in the western and north-west provinces of Cameroon (S. Lamlenn)
IESA/P/AC.21/14	5	Select methodological issues in studying the effect of developmental projects on population (A.I. Hermalin and K. Namboodiri)
IESA/P/AC.21/15	5	Data and measurement problems in aggregate analysis (D. Sly)
IESA/P/AC.21/16	5	Indirect demographic estimation of subnational populations (K. Hill)

Annex III

AGENDA

1. Opening address.

2. Overview of the Workshop:

(a) Assessing the demographic consequences of electrification and irrigation projects using areal analysis;

(b) Application of population impact analysis to project planning:

(i) Experience of Mexico;
(ii) Experience of Cuba.

3. Experiences with population impact analysis:

(a) Experience of the Population Council;
(b) Experience of the Carolina Population Center;
(c) Experience of the International Development Research Centre;
(d) Experience of the International Labour Organisation.

4. Review of studies in progress:

(a) The Girna irrigation project in Maharashtra: progress report;
(b) An irrigation project in the Punjab: progress report;
(c) A rural electrification project in Costa Rica: progress report;
(d) Project in Cameroon: plan of analysis;
(e) Project in Morocco: plan of analysis.

5. Methodological considerations:

(a) Selected methodological issues in studying the effect of development projects on population;

(b) Data and measurement problems in aggregate analysis;

(c) Indirect demographic estimation of subnational populations.

6. Adoption of recommendations:

(a) Recommendations for country studies;
(b) Recommendations for comparative analysis;
(c) Recommendations for further research;
(d) Recommendations for application to planning.

7. Closing remarks

Part two

BACKGROUND PAPERS

I. GENERAL OVERVIEW

Methodological considerations in assessing the demographic consequences of major development projects

United Nations Secretariat*

The World Population Conference, held at Bucharest in 1974, and the International Conference on Population, held at Mexico City in 1984, stressed the importance of the integration of population variables into development planning. The first recommendation adopted in Mexico stated that "national development policies, plans and programmes, as well as international development strategies should be formulated on the basis of an integrated approach..." (United Nations, 1984). However, development planners have given major emphasis, so far, to only one side of the integration process, i.e., population responsive planning activities. In such planning, projected population growth affects decisions with regard to both overall development strategy and inter- and intra-sectoral allocations. Important though such exercises are, the other aspect of the integration process, i.e., population variables influencing components of development plans and programmes having implications for inter- and intra-sectoral allocations of resources, have received much less attention. In particular, very few planning exercises have involved studies that examine the likely demographic outcome of specific development projects.

The relative scarcity of project-level demographic analysis reflects historical circumstances. Usually initiated on the basis of their specific socio-economic benefits (such as improvement in income and/or welfare), development projects are rarely integrated with national population policy goals. Moreover, where surveys have been carried out, few have given sufficient attention to the demographic effects of specific development projects taking place in the area. And even when development projects are initiated or completed, very few follow-up surveys monitor either the development or the demographic impact over time.

The long-term goal of the present study is to assist planners in projecting the likely demographic outcome of projects that are not designed specifically to influence demographic patterns. This is not to suggest that development projects should be assessed primarily on the basis of their demographic impact. Rather, the purpose of this study is to offer the plannersand policy makers a better way of treating the demographic outcome as a factor to be taken into consideration in the design of, or decision to initiate, specific development projects. This is of great importance since the demographic consequences may well feed back to enhance or restrain social and economic improvements. Such consideration should also promote increased consistency between efforts to attain the goals of population policy and those of development policy.

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* Population Division, Department of International Economic and Social Affairs.

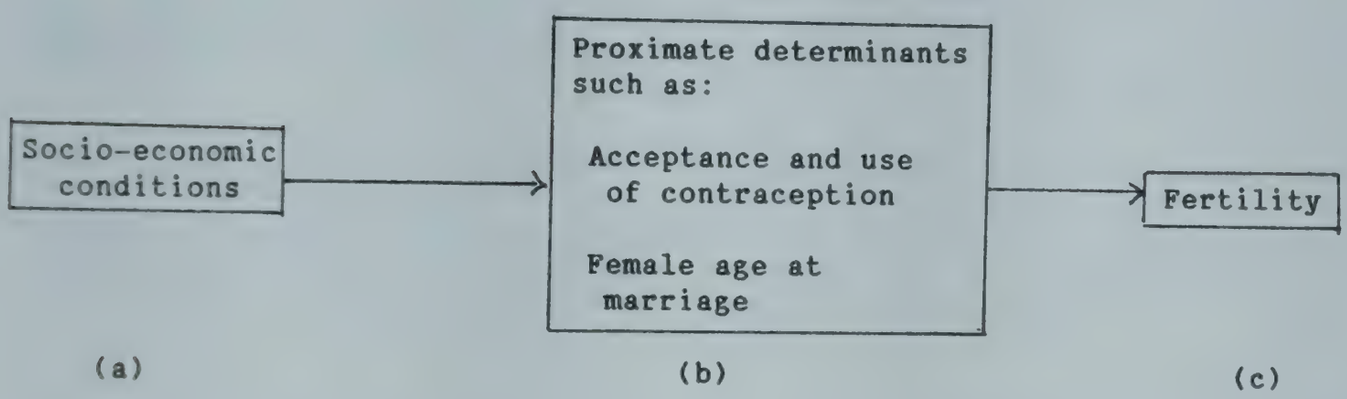
Development project impact studies

In general terms, studies dealing with the socio-economic determinants of population change are similar to studies where the population changes are examined with reference to a specific development project. However, they differ in three significant aspects, which concern their (a) conceptual models, (b) frames of reference, and (c) policy implications. These differences are discussed below.

Conceptual model

A conceptual model linking changes in socio-economic status, to a demographic variable, say fertility, may be represented in simple terms by the following diagram:

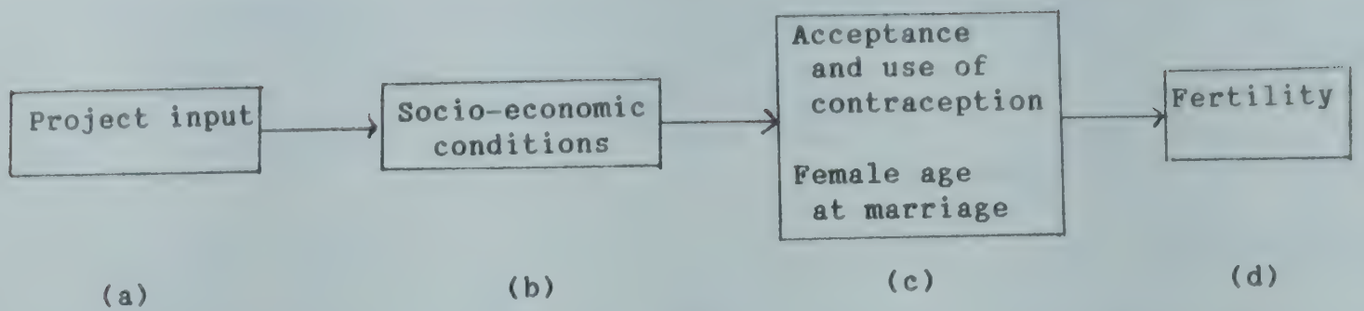
Figure 1



This conceptual model implies that changes in social and economic conditions of the population result in changes in proximate determinants (for example, in family planning acceptance as well as in the female age at marriage), which in turn results in changes in fertility.

As can be observed from figure II below, the introduction of a development project in a particular area changes the social and economic conditions of the population of that area. Changes in the social and economic conditions of the population affect proximate determinants which, in turn, affect fertility. As will be discussed later, the major methodological problem in specifying such a model is the difficulty in ascertaining the extent to which observed changes in (b), i.e, socio-economic changes, are due to the introduction of the project-input and the extent to which these changes are due to exogenous factors. Without being able to attribute changes in the social and economic factors to the introduction of the development project, it will not become possible to conclude that the changes in the demographic variables are the consequences of the development project.

Figure II



Frames of reference

When a specific development project is studied, it defines the time-frame and the geographical area under consideration. This is in contrast to studies of the socio-economic determinants of overall demographic change where the emphasis is on establishing a general relationship between socio-economic and demographic changes without considering the specific project settings or when the particular development project is initiated.

Once an area within the country is identified in the context of a development project, then project planners can examine demographic changes over time and place those changes in the context of other changes also taking place. They can also compare changes over time within the area under consideration with changes in areas outside the specific project area.

Policy relevance

Studies of the policy implications of socio-economic determinants of demographic change have a generalized focus where consideration is given to resource allocation between competing sectors, such as education, health, family planning, labour participation of women etc. In each of these sectors the influence of development projects on population may be taken into account by planners and policy makers. In studies of the impact of development projects, however, the focus shifts from the national level to the sub-national regional level, at which time the proposed location and timing of the project assumes significance. This is because the national perspective aggregates and encompasses very important regional variations in the social, cultural and economic milieu of the various population groups residing in different regions of the country. Development project impact studies, on the other hand, by virtue of their application to specific time and place, invariably recognize and highlight important regional differences. This suggests that whereas the policy-focus of the determinants of socio-economic studies is national, the focus of development project impact studies is sub-national and regional. This important feature of the studies implies a requirement for an experimental design, the implications of which will be discussed in a later section.

The proposed project

In 1985, the Population Division, with funding from the United Nations Fund for Population Activities (UNFPA), initiated a project to assess the demographic consequences of major development projects, in order to examine their nature and magnitude in specific geographical locales. The project's aim is to investigate the extent to which official and project data may be used to discern demographic consequences in five selected countries varying widely in their social, economic and cultural context as well as in the degree of completeness of demographic and socio-economic information available.

The International Institute for Population Sciences at Bombay, India, the Pakistan Institute for Development Economics at Islamabad, Pakistan, and the Centre for Demographic Research (CERED) of the Institute of Statistics at Rabat, Morocco, are engaged in studying the demographic consequences of irrigation projects in their respective countries. The Institute of Economic Growth of the University of Costa Rica at San José, is engaged in examining the demographic consequences of a rural electrification project; and the Centre for Economic and Demographic Researches (CRED) of the Institute of Higher Education at Yaoundé, Cameroon, is engaged in studying the demographic consequences of rural agricultural co-operative projects.

The methodological approach proposed for application in the five case-study countries is based on the following general considerations:

(a) It should address the practical concerns of the planners with regard to the possible demographic outcomes;

(b) It should be scientifically sound;

(c) It should have applicability to a wide range of developing countries.

In presenting such an approach, this paper will discuss some key issues that have been raised in the literature. The first is the selection of the particular development project to be studied; second, the choice of the demographic variable (or variables) to be examined; third, the consideration to be given to the level of analysis as well as the research design; and finally, the specific data required to make a linkage between the development project and demographic outcome. The kind of lesson that may be learned from the five case-studies will be specified.

Selection of a development project for assessing its demographic consequences

Development projects can be classified according to any of three criteria: the socio-economic characteristics of the target population; their welfare goals; and how the benefits are to be distributed.

In the first instance, for example, a development project may be primarily targeted to the rural population, the urban population or a combination of both. If a project is targeted exclusively to either the rural or the urban population, the fertility and mortality changes in the project area can be studied and compared with events in areas where no similar project exists. However, if a project is designed to benefit an area that includes both rural and urban population, fertility and mortality changes may need to be considered for rural and urban populations separately. For migration, however, in addition to examining the migration patterns to and from the project area, migration between areas within the project locale may have to be taken into account.

In the second case, development projects may be classified according to their welfare goals. Some projects are directed to raising the level of income or output, whereas others are primarily oriented to improving health or educational levels. Integrated area development programmes generally aim both at raising income and improving the health and education of the population in a specific geographic area. The methodological approaches for assessing demographic consequences may not differ for projects primarily oriented towards raising income or those primarily oriented towards improving the status of health and education. In the case of integrated area development projects, however, since a large number of development inputs are considered to be operating on a group of population, applying an appropriate methodology, in the absence of an available conceptual framework, may be a formidable task.

Applying the third criterion, it may be important to distinguish between projects according to the way the derived benefit is distributed to the population. If a large proportion of the benefit is enjoyed by a very small proportion of the population, the development project may not result in as significant a change as would have been the case were the benefits flowing from it evenly distributed. For example, in the case of electrification projects, it may be important to determine whether the project has been able to distribute energy to a large proportion of the households. Similarly, if an irrigation project is initiated to raise the productivity of the land, it may be important to know who actually benefits from the increased productivity. If a very large proportion of the agricultural land is owned by a very small proportion of the total population (including absentee landlords residing in urban areas), then the benefits of an increase in productivity may not be distributed to the population of the area. Put differently, traditional social and economic structure mediates between the development input and the degree to which the benefits flowing from such an input are distributed to the population. Therefore, simply considering an overall increase in productivity resulting from the irrigation project may not reveal the underlying features of the society: a deeper understanding of the social structure and its institutions is needed in order to interpret the observed relationship between development input and its demographic consequences.

Review of the relevant literature on the demographic impact of development projects suggests that the rural electrification-fertility relationship is the most commonly studied research problem. Out of 17 studies

considered, 10 dealt with electrification and 2 studies each dealt with irrigation, integrated rural development programme and cottage industries. One study was found that dealt with agricultural co-operatives (see table 1).

It is apparent from the above review that large development projects directed to the rural population are generally chosen for a study of demographic impact. Given the fact that about 70 per cent of the population of the world are rural, poor and agriculturally based, it makes a great deal of sense to focus on development projects targeted to the rural population.

It would have been preferable to have chosen one type of development project, such as an irrigation project, for study in all the five countries. In such a case, a comprehensive evaluation would be possible of the role of social and cultural factors mediating between the irrigation project and the observed demographic outcome across the five countries under consideration. However, the developmental context and availability of the required data base, as well as the need to select countries representing a wide range of variability in regional, social and cultural conditions, led to the final selection of irrigation projects in Costa Rica, India and Pakistan, a rural electrification project in Costa Rica and agricultural co-operatives in Cameroon. The common element in all these projects is that they are directed to improving the economic conditions of the rural population.

Selection of the demographic variables

In addition to the considerations stated above, the selection of the development project is also linked to the choice of the dependent demographic variable whose effect is being measured.

As a result of an increase in the economic opportunities, there may be fertility, mortality and migration consequences. An array of development projects can be visualized that are primarily directed to improving the level of income of the population under consideration. Changes in the economic conditions of the population may also lead to significant changes in the social organization, resulting in a shift from household or village-centred productive activities to productive activities that are part of a much larger market economy of the region. Increased economic opportunities may result in specialized work away from home, leading to migration. In such an event, there may be a decline in economic activities where children can participate. Other things remaining equal, improvement in income may result in decreased reliance on children as an investment, as well as for old-age security. Finally, there may be an expansion in economic opportunities for women, resulting in increased opportunity costs of child-rearing, as well as a significant gain of power by women. In addition, improved economic conditions of the population may also have implications for mortality (especially infant mortality).

The review of previous studies (table 1) suggests that fertility (and in some cases contraceptive prevalence) is the demographic variable most commonly examined for assessing the consequences of development projects. None of the studies reviewed dealt with the effect of development projects on mortality; and only two studies examined the migration effect.

Table 1. Summary of empirical studies on assessing the demographic consequences of development projects

Place and time of study	Type of project	Time interval between introduction of project and measurement of demographic consequences (years)	Measure of development project	Unit of analysis and sample size (households: h/h)	Measure of demographic behaviour	Method of analysis	Conclusions
(1) Misamis Oriental, 1979	Electrification	1-9	Presence or absence of electricity in village household	10 electrified villages 10 non-electrified villages covering 4,168 eligible females	Fertility: births per women in past 5 years and past 2 years Current contraceptive practice	Comparison of rates: electrified vs. non-electrified Regression analysis	Electrified villages had 20% lower fertility and 18% higher prevalence Standardized beta coefficients for impact of electrification of household of -.386; and for electrification of village to current contraceptive practice of -.124
(2) India, 1960-1970	Electrification	Not stated	Per capita electricity consumption	16 states	Crude birth rate CPR	Stepwise regression	No significant relationship between electrification and crude birth rate. CPR and electrification shows an R^{-2}=.61
(3) North-east Thailand, 1980	Electrification	5	Presence or absence of electrification in village	300 h/h in each of 2 villages	Cumulative fertility Recent fertility CPR	Comparison of rates between electrified and non-electrified households	In electrified villages (a) CPR is 20% higher; (b) Cumulative fertility is 20% lower; (c) Recent fertility is 23% lower.
(4) Thailand, 1979	Electrification	10	Presence or absence of electricity in village	2,136 h/h	CPR	Path analysis	When presence or absence of electrricity is controlled, the impact of contraceptive availability on CPR is reduced by half
Thailand, 1979	Electrification	10	Houses electrified (%)	64 villages	CPR	Path analysis	Of the 57% of the variance explained, 39% was accounted for by the direct path from electrification to CPR

Table 1 (continued)

Place and time of study	Type of project	Time interval between introduction of project and measurement of demographic consequences (years)	Measure of development project	Unit of analysis and sample size (households: h/h)	Measure of demographic behaviour	Method of analysis	Conclusions
(5) North-east Thailand, 1981	Electrification	Not stated	Presence or absence of electricity in villages	4,986 h/h in 16 provinces	CEB CPR Ideal family size	Comparison between energized and non-energized villages Path analysis	Electrified villages had 5% lower CEB 10% higher CPR 7% lower ideal family size
(6) Korea, 1980	Electrification	20	Length of time village is electrified	1,136 h/h in 96 villages	CEB Children recently born Ideal family size	Regression analysis	Electrification had a very small non-significant effect on the dependent variable when only community level variables considered; also when individual level and community level variables included.
(7) USA, 1940-	Electrification	Not stated	Farm households electrified (%)	48 states, 1940, 1950, 1960 and 1970, pooled	Children population ratio	Multiple regression	Standardized beta coefficients for impact on child-population ratio 1940:-.27 1950:-.10 1960:-.25 1970:-.31 Pooled:-.34
(8) USA, rural south, 1930-1950	Electrification	Not stated	Farm households electrified (%)	716 rural counties: 1920, 1950, 1960	Children/women ratio	Multiple regression	Standardized beta coefficients for impact of electrification on child/women ratios: 1930:-.13 1940:-.22 1950:-.13
(9 Bangladesh, 1982	Electrification	3	Household electrified or not	600 h/h in 24 villages; 200 h/h with electricity	Completed family size Ideal family size	Comparison of means and rates	Electrified households had: 10% lower completed fertility 10% lower ideal family size No difference in CPR

Table 1 (continued)

Place and time of study	Type of project	Time interval between introduction of project and measurement of demographic consequences (years)	Measure of development project	Unit of analysis and sample size (households: h/h)	Measure of demographic behaviour	Method of analysis	Conclusions
(10) Colombia, 1980	Electrification and education	4	1 town electrified only; other village has educational facilities as well	All households in both towns interviewed Electrified villages 80 h/h, electrified 110 h/h fewer	In and out migration from each of the two villages during the past 10 years	Logit regression model	Electrification plays an important role in the retention of population Extension of education may have helped retention of young people aged 15-19 in the second village
(11) Sri Lanka, 1975-77	Irrigation cum resettlement project (Mahaveli River Project	6	Whole island population considered to be covered by project	Census population figures of 1970/71 socio-economic survey of Sri Lanka	Fertility: Contraceptive acceptance rate Mean age at marriage Mean number of live births	Cross-population logit regression model	The project would increase births by 18 per 100 women. This represent 3.7% increase in completed fertility
(12) Northern Thailand, 1975-79	Irrigation	Not stated	Irrigated vs. non-irrigated villages	2 non-irrigated villages (123 h/h) 3 irrigated villages (385 h/h)	In and out-migration Average number of children surviving	Cross population average between households of irrigated vs. non-irrigated villages	Out-migration from areas adjacent to the irrigation; In-migration to irrigated areas Higher levels of morbidity and mortality in non-irrigated areas
(13) North-eastern Thailand, 1981	Agricultural co-operatives	Not stated	Households belonging to 4 agricultural co-operatives ranging from very successful to unsuccesful; also a control group of non-co-operative households	873 h/h	Fertility: number of children ever born	Multiple regression	Cumulative fertility higher among members; but number of children born in the last 3 years is lower for members compared to non-members

Table 1 (continued)

Place and time of study	Type of project	Time interval between introduction of project and measurement of demographic consequences (years)	Measure of development project	Unit of analysis and sample size (households: h/h)	Measure of demographic behaviour	Method of analysis	Conclusions
(14) Comilla, Bangladesh	Integrated rural development project	25	All population within the project enumerated	Individual	Fertility Age-specific fertility rate Maritial fertility rates	Multiple regression	Fertility declined in the project area; but decline was more than compensated by reduced infant mortality
(15) West Bengal, 1979	Cottage industry	20	Participation or not cottage industry	Households: sample of 362 families representing both participating and non-participating families	Fertility: children ever born	Cross-tabulation multiple regression	Participation of children in the industry is made reluctantly. Higher fertility is one of the reasons for sending children to work in the industry and away from school
(16) Rural Thailand 1980-81	Cottage industry	Not stated	Sample of households of an area covering all economic activity	247 h/h	Fertility: children ever born	Multiple regression	Generally speaking female labour force participation is negatively related to fertility
(17) Bangladesh	Integrated rural development programme	25	Households of village within the purview of the development programme	Households: 227 Individuals: 1,446	Nuptiality Contraceptive use Fertility	Cross-tabulation	Fertility lower in the village as compared to the national average

Source: Sarah F. Harbinson and Warren C. Robinson, "Rural electrification and fertility change", Population Research and Policy Review, vol. 4, No. 2, table 1, pp. 162-164, revised and updated by the Population Division according to sources in the text.

From a methodological point of view, neither fertility, mortality or migration differentials can be analysed without some reference to the other two processes. For example, in order to attribute observed changes in fertility to a specific development project, one will need to know the role that migration, as well as mortality, has played on the observed fertility changes. Nevertheless, in this study, the principal focus will be on fertility, even though all demographic effects must be considered.

The decision to emphasize fertility was based on the following considerations:

(a) Considerable time and resources will be necessary to consider all the three demographic processes simultaneously. By focusing primarily on fertility, the present study will, however, make contributions towards the broader goal of a generalized model of demographic change;

(b) Fertility is the most intensively studied demographic factor. By emphasizing the fertility effects, the study will benefit from the findings of a broader range of other studies. In particular, there is a well developed literature in the proximate determinants of fertility. The proximate determinants which link change in socio-economic condition to changes in migration and mortality are not yet as well understood;

(c) The Population Division has completed a study on measuring the impact of family planning programmes on fertility (United Nations, 1978). Focusing on fertility will allow the project to draw upon the important lessons learned from that study. Moreover, in the case of fertility, information with regard to at least two proximate determinants (i.e., family planning acceptance and female age at marriage) are more readily available for the developing countries. Therefore, while assessing the fertility consequences of development projects, the project will not only attempt to ascertain whether significant fertility changes have taken place but attempt to explain the process by examining the relative contributions of two proximate determinants of such fertility changes. In the case of mortality and migration, the project will only examine whether significant changes have occurred in them as a result of the introduction of the development project.

Selection of a methodology

There are many facets to the various methodological approaches advanced for assessing the demographic consequences of major development projects. The task of developing such a methodology is a challenging one since it goes a step beyond a methodology for measuring the impact of family planning. In this case, other development inputs must be brought into the picture.

In the case of the development project studies (see figure II), one can trace the linkages between development project input to changes in socio-economic status; changes in socio-economic status to changes in proximate determinants; and, finally, changes in proximate determinants to changes in fertility.

At any particular point in time, many developmental project activities operate simultaneously in a given area. As a result, it will be necessary to ascertain the extent to which observed changes in the socio-economic variables are due to the project input and the extent to which these changes are due to other development inputs that are not considered. Without linking observed changes in socio-economic variables to the specific development project, it will not be possible to conclude that changes in the demographic parameters are the consequences of the development project.

Assuming that it is possible to establish a true experimental design, one can measure the effect of the project under consideration without necessarily understanding the process linking the project to a demographic outcome. However, the research design should deal with the issue of pre-selectivity and control, as well as with the issue of the unit of analysis. They are discussed below.

Issue of pre-selectivity and control

It is necessary to obtain a historical perspective of the area where the development project is located. For example, the unusual nature of the population may have been a factor in the decision to locate the development project in a particular area. It is possible that population groups in the project area were more mobilized than others and were able to generate effective political and social pressure, which resulted in more development resources being allocated to their region. If so, it may be that this group was more predisposed towards change, including demographic change, than the population outside the project area. Thus, one may actually be assessing the demographic effect of these social and political features, instead of the development project as such. This important contextual factor should be appropriately taken into account.

A major challenge in designing an experimental study, therefore, lies in establishing comparability or equivalence between the experimental and the control group. Randomly assigning population to either the experimental or the control group ensures that the sampling is from the same population, i.e., the control and the experimental group differs only by the sampling error.

In undertaking an evaluation of the demographic impact of a development project, one will rarely find two population groups identical in every respect at the time when the input was given to one group and not to the other. Therefore, the research design must take into consideration the distribution of various social, economic and other development inputs in the experimental area where the specific development project is located as well as in the control area where the treatment variable is absent. Once this information is available, statistical analyses make it possible to establish comparability between the experimental and the control group by properly handling the "disturbing variables", the variables that are not being studied explicitly but which disturb the comparability between the two groups.

The effect of the disturbing variables can be controlled either by matching or by adjustment. In the case of matched design, communities or groups of population are divided, depending on the presence or absence of the development input. Then communities or groups can be selected by matching of the disturbing variables. The statistical method of matching involves cross-tabulation of both groups by all disturbing variables and selection of the number from each cell for the control group corresponding to the number in the experimental group.

In the case of controlling by adjustments, unlike matching, no attempt is made to balance numbers. Instead, means are calculated for each cell and a common set of weights is used to calculate an adjusted mean for each group. This is commonly known as adjustment of sub-class differences. Another type of adjustment, known as co-variance adjustment, involves fitting a common (multiple) regression function to the disturbing variables to determine the adjustment factors required to compensate for differences between the two groups.

Another way of establishing comparability is to divide the population groups into sub-groups corresponding to high or low fertility. The population from high fertility groups are matched or adjusted using the principle suggested above.

The Bangladesh study (Harbison and Robinson, 1985) establishes comparability by randomization. This enables them to assume that the distribution of the disturbing variables is similar in both the experimental and the control area. Herrin, taking it one step further in his Philippine study, tests for selection bias using regression analysis in order to determine whether the project area selected differs systematically from the non-project area in basic background characteristics (Herrin, 1979). In order to continue the analysis it had to be demonstrated that the coefficients of the areal-level characteristics of the two populations are not significantly different from each other.

Mueller and Anderson, 1982, in their study of the Comilla Programme in Bangladesh, treated the whole country as the control group against which they compared an area covered by the Comilla Programme. They tested for selection bias by comparing the socio-economic characteristics of the whole country with the area covered by the Comilla Programme at the beginning of the implementation of the project. Another approach taken by some researchers (e.g., Prasartkul, 1980) to ensure comparability is to select areas adjacent to the irrigated area as the control area. Here, the assumption is that adjacent areas are similar in various social economic and other developmental parameters.

The role of the "control" area is to take into consideration the other factors such as health, education, the status of women, the value of children, and income which affect fertility independent of changes due to the introduction of the development input. One can treat areas with or without development projects as constituting the experimental and control group, and

one can test for differences in the means of the demographic values. Control may also be established by allowing the experimental units to serve as their own controls by following a "before and after" design.

In summary, the research design, being experimental, must take into account the problem of pre-selectivity and the problem of internal comparability. By examining for the problem of pre-selectivity one attempts to ensure that the variables not considered in the study do not influence the interpretation of the results obtained. Once this has been established, then one tries to ensure internal comparability between the two groups based on the variables for which data have been collected.

Unit of analysis

Changes in demographic behaviour may come about as households respond to development inputs. Thus, household level analysis is required in order to fully understand the process linking development inputs to a demographic outcome. However, infrastructural or other community level changes, irrespective of whether the entire population benefits from the development inputs or not, are also significant agents of change for the population as a whole.

For the purpose of the present study, it was decided to employ, as the units of analysis, geographic areas instead of individuals or households. Though there are significant advantages in selecting individuals or households (as opposed to areas) as the units of analysis, one of the criteria of the proposed methodological approach is that it should be useful to planners and at the same time impose minimum requirements for collection of additional data. In a great many countries, the largest and the most readily available sources of data are areal rather than individual in nature. Moreover, some promising lines of demographic research suggest that (for some important independent variables) the most useful unit of analysis may not be the individual or the household, but rather the local community or some other aggregate unit that may be described by areal data.

One cannot automatically assume that the relationship observed using the community as the unit of analysis will also hold true at the level of the household. Care must be taken to avoid what is known as the "ecological fallacy". However, irrespective of the unit of analysis chosen for the study, a significant association between development inputs and demographic change is generally observed. Ideally, however, "multi-level" analysis, as proposed by Hermalin (Hermalin, 1986), is most efficient. In such a case, data from the community level are combined with data from the individual household level which makes it possible for analysis to be conducted on the relationship between levels.

A review of the studies presented in table 1 suggests that except for studies in India, Thailand and the United States of America, where geographic areas are the units of analysis (items 2, 4, 7 and 8), most studies have households as the unit of analysis. As to be expected, in cases where areas

are the units of analysis, information is collected from the available sources, for example, censuses, development project evaluation units etc. In the studies reviewed, where households are the units of analysis, the data are derived from surveys either conducted for some other purposes, or specifically organized for the study under consideration.

Although a carefully done retrospective household survey can be efficient in collecting reasonably accurate data on pregnancy histories, it will be very difficult to obtain corresponding social and economic data at the household level. Yet without being able to link social and economic data with demographic data, before and after the introduction of the development project, the impact of development inputs on the demographic variables cannot be measured.

Moreover, since very little is empirically known about the time lags, i.e., after how many years demographic changes based on development changes can be observed, trend data must be collected over a period of time. One way this can be done is by undertaking a prospective study, using households as the units of analysis. However, given the time-lag between a development project and fertility change, it may be necessary to follow through with the household for a long period of time. On the other hand, areal level analysis, where the village, country or district is the unit of analysis, does lend itself to collecting social, economic and demographic data, over time, before and after the introduction of the development project.

The greatest advantage of such an approach is that it allows use of the census, the main institutionalized source of social, economic and demographic data. Moreover, information routinely collected by the project monitoring cell of the specific development project, as well as vital registration, may provide additional useful information.

Areal level analysis also enables formation of an integrated picture of all three demographic processes. Though the primary focus of the study will be on fertility, this approach allows examination of mortality and migration changes as well. More specifically, out-migration is an important demographic process to be taken into account when examining the consequences of a development project in a rural area. Household level data cannot provide an adequate picture of migration. With census data, at two points in time and with fertility and mortality estimates, it is possible to derive indirectly, patterns of inter-censal out-migration.

Census organizations and similar administrative machinery are at different stages of effectiveness and efficiency in different countries of the developing world. Therefore, an areal level approach based on existing available data over a long period of time will not be feasible for many developing countries. In those cases, conducting a household survey may be the only available course of action. However, it can be expected that in those countries the effectiveness of various data collection and administrative institutions will improve over time so that in future they also may be able to make use of the methodological approach under consideration.

The outline of the methodology proposed is presented in table 2. Column 1 of that table refers to units of observation, which can be a village, a county or any other areal unit within the geographical area of the project input. Column 2 refers to establishing fertility, mortality and migration values at a time when a census was taken. Column 3 refers to the development projects that had taken place prior to the time when the development project under consideration was introduced. For each project, information should be obtained regarding how long ago that particular input was given and, if possible, the extent of the input (for example, in the case of irrigation, the proportion of the agricultural land irrigated).

Column 4 refers to the time when the development input was given and the extent of the said input. Column 5 refers to the measurement of fertility, mortality and migration values subsequent to the introduction of the development input whose demographic effect is being measured. Column 6 refers to development inputs that are given subsequent to the project input under consideration. Column 7 refers to the measurements of demographic values at time t_3. Therefore, the net demographic effect will be the difference between column 7 and column 5 in both the experimental and control group; and the difference in demographic values between them can be attributed to the development project.

Proposed data sets

A review of the range of social, economic and demographic information collected by censuses in the various countries of the world is presented in table 3. As can be observed from the table, for a large number of countries a wide variety of social, economic and demographic information is available for at least two census points in time. However, this information may not be available at the levels of disaggregation needed for project impact analysis. Owing to resource constraints, not all information collected through censuses is routinely tabulated at all levels of disaggregation.

Based on field visits to the five case-study countries as well as consultations with respective country consultants, a set of major variables has been identified as variables through which planners and policy makers attempt to raise the income as well as the general well-being of the rural population.

The list was developed mainly with reference to the area of the specific development project whose impact is being studied. It is quite possible and perhaps likely that for some countries not included in this study, other variables may be more important. Although the degree of detail as well as the time-frame differs among the countries being considered, an idea of the range of data to be considered can be obtained from the matrix shown in table 4.

It has been proposed that there should be at least 40 units (areal) of observation for which all the above socio-economic and demographic information is collected for different points in time; moreover, that another similar set of at least 40 units of observation should be obtained from an area falling outside the area of the project input, to serve as a control group. This data set provides a range of analytical possibilities each with its own advantages and disadvantages.

Table 2. Outline of the research design to study the demographic impact of major development projects

(t = time census taken)

Units of observation (1)	Demographic values at t_1 (census years) (2)	Inputs existing prior to introduction of specific development project (3)	Project input at t_2 (t_1+10 years) (a) Number of years (b) Extent of input (4)	Demographic values at t_2 (5)	Other development inputs subsequent to introduction of project input (6)	Demographic values at t_3 (t_2+10 years) (7)	Demographic effect (8)
Experimental group							
1							
2							Col. 7 - Col 5
3							
4							
5							
.							
.							
Control group							
1							
2							Col. 7 - Col. 5
3							
4							
5							
.							
.							

Table 3. Distribution of countries by selected social, economic and demographic information available for the period 1970-1980

	Africa		South America		North America		Asia	
	1970	1980	1970	1980	1970	1980	1970	1980
gration								
. Place of birth	32	37	12	11	34	26	23	22
. Duration of residence	13	17	7	6	26	25	19	2
. Place of previous residence	5	12	5	4	18	15	13	13
rtility, mortality								
. Children born alive	22	31	11	8	31	23	19	21
. Children living	21	29	8	8	8	9	18	17
. Live births in past 12 months	19	33	7	7	22	17	7	15
. Deaths of infants born in past 12 months	9	26	1	3	6	4	4	7
ucation								
. Educational attainment	33	38	11	10	34	27	28	25
. Literacy	18	26	10	9	14	12	25	20
. School attendance	24	30	11	9	30	24	16	20
usehold families								
. Marital status	30	35	12	11	34	27	30	29
onomic								
. Activity status	30	39	12	8	34	25	29	26
. Occupation	30	38	12	8	34	24	29	26
. Industry	29	34	11	8	34	25	29	26
. Status in employment	25	37	11	8	34	25	29	22
mber of countries	(36)	(39)	(12)	(11)	(34)	(27)	(30)	(29)

Source: Statistical Office of the Department of International Economic and Social airs of the United Nations Secretariat.

Table 4. Hypothetical matrix of data required for each of the units of observation

Variable	Year of observation t_1(1960)	t_2(1970)	t_3(1980)	t_4(1985)
A. Socio-economic				
1. Income/standard of living				
2. Irrigation (including distribution of land ownership)				
3. Electrification				
4. Health and family planning				
5. Education				
6. Transportation				
B. Demographic				
7. Population size				
8. Fertility (including nuptiality, for example percentage of females married in different age groups)				
9. Mortality				
10. Migration				

Socio-economic variables (table 4, 1-6)

For each of the socio-economic factors, efforts are being made to collect as much information as possible. More specifically, the consultants have been urged to collect the following type of information:

(a) Income or a proxy. If a direct measure of income is not available, some proxy, such as type of dwelling or percentage of hired labour in the population;

(b) Irrigation

(i) Area sown: once/twice;

(ii) Proportion of households owning irrigated/unirrigated land;

(iii) Proportion of land irrigated from other sources;

(iv) Proportion of households and economically active population engaged in agriculture;

(c) Electrification

(i) Whether the district is electrified or not;

(ii) For the electrified district, proportion of households electrified;

(iii) Purpose for which the electricity is used (in percentage of electricity used in the district): domestic (households, shops, farms); industries; other (specify);

(d) Health and family planning variable

(i) Nearest distance from the district to each of the following:

	In the area	Less than 3 miles	3 - 10 miles	More than 10 miles
Hospital				
Dispensary				
Trained midwives				
Family planning clinics				

(ii) Predominant source of drinking water supply:

	In the area	Less than 3 miles	3 - 10 miles	More than 10 miles
Piped water				
Tube well				
Surface well				
Tank				
River				

(iii) If family planning services provided, number of acceptors

(e) Education variable

	In the area	Less than 3 miles	3 - 10 miles	More than 10 miles
(i) Proximity of nearest school				
Primary				
Secondary				
(ii) Student enrolment ratio (Male/Female)				

	In the area	Less than 3 miles	3 - 10 miles	More than 10 miles
(iii) Proportion of children attending school from other districts				
(iv) Adult literacy rate				
(f) Transportation/communication variable	In the area	Less than 3 miles	3 - 10 miles	More than 10 miles
(i) Nearest motorable road				
(ii) Nearest railway station				
(iii) Nearest bus station				
(iv) Nearest post office				
(v) Nearest telegraph office				
(vi) Nearest town				

Demographic variables (table 4, 7-10)

With regard to the demographic variables, ideally one should have data on age-specific fertility and mortality (including infant mortality), information on breast-feeding and nuptiality, as well as on migration for each of the units of observation at different points in time. Often such information may not be routinely available at the sub-national level. It may be necessary to estimate various demographic measures indirectly based on what is available for the sub-national level at the unit of disaggregation. However, since in sub-national demographic estimation one cannot assume a closed population, migration should be estimated before estimating fertility and mortality.

In addition, information regarding the history of the various developmental inputs in the area will be collected. With regard to the socio-economic variables listed above, the following additional information on timing is necessary:

(a) Education variable. When was the school (primary and secondary) established?

(b) Electrification variable. When was the area electrified?

(c) Irrigation variable. When was the area irrigated?

(d) Health and family planning. When was the health centre and/or family planning services established?

Since most of the information that could be collected, short of launching an actual survey, are in the process of being put together in each of the five case study countries, the data matrix will represent a large portion of the possible information available. The task of the project is to draw conclusions linking specific development projects to demographic outcomes based on that body of information.

Conclusion

Recognizing that planners have repeatedly been urged to take into consideration the interrelationships between social development, economic development and population as it applies to specific development input, this project examines methodological issues that confront planners in assessing the demographic consequences of development projects. The effects of development projects on demographic processes are complex, and our understanding of the causal mechanisms are limited. Nevertheless, planners must make decisions based on an imperfect understanding of such mechanisms.

In order to capture some of the major associations between development intervention and demographic variables, a simple conceptual framework of a few variables has been proposed for assessing such demographic consequences. The results of the country studies should indicate the usefulness of this approach. Moreover, the conceptual framework assumes that the social processes generated by development interventions, such as irrigation or electrification, are similar even in countries having different social, economic and cultural backgrounds. The case-study results will indicate the degree to which this assumption is valid and whether it becomes possible to pinpoint the components of these variables which will be most relevant for influencing the demographic processes.

The methodological approach proposed for the project has the following features: (a) the analysis is based at an aggregate level; (b) the data-set required is either generally available or could be estimated at the sub-national regional level; and (c) the available data will conform to the requirements of an experimental design. The five case studies currently under way should assess the strengths and limitations of this approach.

More specifically, it is expected that the five country case studies will throw light on the following aspects of the project methodology:

(a) The country case studies should provide important lessons indicating the nature of difficulties in drawing conclusions based on the aggregate-level data, how to avoid problems of the ecological fallacy and to correctly interpret the results;

(b) The case studies should show to what extent it becomes possible to establish comparability between the experimental and the control group and how comparability affects the results obtained;

(c) The design for the project assumed that the data-set of the type proposed for the study is readily available and usable at the sub-national regional level. The country-experiences should confirm the validity of the assumption;

(d) Finally, the results obtained from the studies should indicate the type of practical use they have for planners, either in the short term or in the long term and also how this contributes to integration of population variables into development planning.

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II. EXPERIENCES OF OTHER INSTITUTIONS WITH POPULATION IMPACT ANALYSIS

A. Impacts of development projects on fertility in Asia

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Formulation of development policies and their implementation through programmes and projects are primarily concerned with issues related to the social and economic development of a country; second-order effects on the demographic parameters are rarely considered. Special population policies are designed and implemented, usually through family planning programmes, to address the issues of rapid population growth in developing countries. Following the World Population Conference of 1974, work in the field of population generally and that of a number of international organizations concerned with population passed through a period during which the basic premise underlying population activities was reexamined. This led to a number of attempts to integrate population concerns fully into development planning.

In 1976, for example, the Foreign Assistance Act was amended by the United States Congress to include section 104(d), which states:

> "Assistance under this Chapter shall be administered so as to give particular attention to the interrelationship between (a) population growth, and (b) development and overall improvement in living standards in developing countries, and to the impact of all programmes, projects, and activities on population growth...".

The United Nations Fund for Population Activities (UNFPA) initiated steps to establish population units in planning organizations in order to facilitate the incorporation of population concerns in development activities.

One of the major programme initiatives undertaken by the Population Council in the late 1970s, in response to perceived needs of the field, was a programme on the Fertility Impacts of Development (FID) in Asia. The overall objective of this programme was to generate new knowledge about the impact of socio-economic development on fertility and to assist policy makers and programme managers in incorporating the implications of development on fertility in the planning process.

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Four types of activities were undertaken to achieve this objective:

(a) A studies programme to support research by Asian scholars on the quantification of the fertility impacts of development processes and programmes;

(b) A series of policy and technical workshops to stimulate research proposals, to share research experience, and to stimulate interactions among the researchers and policy makers;

(c) Provision of technical assistance to development planners and policy makers in Asia, in order to develop programmes that give explicit attention to their fertility impacts;

(d) Dissemination of information to researchers and policy makers in Asia through publications resulting from studies, workshops and technical assistance activities.

Under the studies programme, 53 proposals from Asian scholars were received, 9 of which were funded. Nine workshops were held: four were oriented to stimulate interactions among the researchers and policy makers; three were primarily for researchers, to stimulate research proposals or to share research experience related to the specific workshop topics; and two were held to review the progress and analysis of research projects funded under the studies programme. In five countries, technical assistance was provided to missions of the United States Agency for International Development (USAID) to review and assess the population impact of their development portfolios; in Bangladesh and Nepal this work included an assessment of the country's development strategies and, in India, an analysis of the country's population policy and programmes was also undertaken.

It was originally hoped to produce a regular newsletter, directed to Asian development planners, programme managers and others in a position to apply research results to actual projects. This proved to be impractical because of the paucity of operationally relevant findings emerging from other sources and the unavailability of findings from FID-supported research. Information dissemination activities, therefore, included the publication and distribution of (a) proceedings of seven workshops; (b) working papers based on assistance provided to the USAID missions in Bangladesh, India, Nepal and Thailand; (c) reports of the selected research projects; and (d) an edited volume published in 1986.

The volume, edited by Stoeckel and Jain (1986), is based on the results of five selected studies funded under the studies programme. Three of them, two in Thailand and one in Sri Lanka, focused upon the fertility effects of interventions to improve economic conditions: agricultural irrigation, rural electrification, land settlement and a guaranteed minimum price scheme. The remaining two projects examined the fertility effects of female employment in industrial and nonindustrial settings in the Philippines, and the fertility effects of child labour and schooling in India. In addition to these five empirical studies, an opening chapter in the volume presents an overview of

the major findings of these studies and their implications for population policy and further research; and a concluding chapter discusses the methodological issues encountered in attempting to assess the fertility impacts of development.

The present paper, based on this volume, briefly discusses the conceptual framework, research design, and level of analysis utilized in the fertility impact studies, summarizes their major findings, highlights some of the methodological issues faced by researchers in assessing the fertility impacts of development, and discusses implications for further research and population policy.

Approaches to assessing impact

Research activities related to the impact of development on fertility are quite similar in nature to those related to the determinants of fertility. There is one important difference, however. The investigator in the determinants type of research moves backward in orientation from fertility to its determinants and then to public policies and interventions. The orientation in the impact type of research, on the other hand, is forward from public policies and interventions to their effects on the determinants of fertility and then to fertility itself. The selection of the determinants to be used in the impact research as intervening factors between public policies and fertility, however, has to be based on the state of the determinants research.

The impact studies supported under the FID programme placed an emphasis on the quantification of the effect of development processes and projects on fertility. The quantification of the impact at the project level rather than at the policy level received additional emphasis for two reasons: (a) development policies, in general, are implemented through specific programmes and projects; and (b) implementation of a policy and, therefore, its effectiveness is likely to vary from project to project. Thus, a policy concerning land reform, for example, might be considered to have important fertility implications in a country; but unless it is implemented in an area, its actual effect on fertility change cannot be quantified.

The impact of development on fertility can be assessed prior to implementing a development project through a fertility impact appraisal, and after its implementation through a fertility impact study. A fertility impact appraisal, similar in concept to economic or environmental appraisals, refers to the projection of potential fertility effects of inputs provided through a development project (see Barlow (1982) and Jain (1986) for illustration of methodologies that can be employed for undertaking fertility appraisals of development). A fertility impact study, on the other hand, refers to the measurement of actual fertility effects of inputs provided through a development project. Other examples of this approach can be found in the volume edited by Bilsborrow and DeLargy (1985), and in the work of Mueller and Anderson (1982).

Conceptual framework

The conceptual framework selected for assessing fertility impacts of development is presented in the following diagram:

Development Project outputs -----> Socio-economic Structure -----> Proximate Determinants -----> Fertility

In general, the impact of development project outputs upon fertility is viewed as indirect, operating through changes in the socio-economic structure that affect the demand for children. By improving social and economic conditions and by creating a demand for consumer goods and social services. The development process is viewed as creating a demand to reduce fertility. These changes in the demand structure are translated into actual fertility through changes in the intermediate variables or the proximate determinants, especially through increased use of contraception (see Davis and Blake (1956) for the list of 11 intermediate variables; and Bongaarts (1978) for the four most important proximate determinants). This framework, however, does not include population projects that can directly influence proximate determinants of fertility. For example, provision of family planning services in an area can increase the use of contraception and decrease fertility without changing the socio-economic structure and demand for children. Assessment of the fertility impact of such projects was not supported under the FID programme.

The five impact studies utilized either all or a portion of this framework to generate hypotheses and guide their analyses. The study in Sri Lanka on the fertility effects of electrification, land settlement and the guaranteed minimum price scheme (and their combinations), and two studies conducted in Thailand on fertility effects of electrification and irrigation, utilized the total framework. These studies dealt with the government-sponsored infrastructural projects, and their output was measured by the presence of these facilities in rural communities.

The remaining two studies used a portion of the framework. These studies did not specifically deal with the presence of a particular government-sponsored infrastructural facility. Instead they started with the output of various governmental and non-governmental efforts to improve economic and social conditions. The Philippine study started with the developmental setting (industrial and non-industrial) of female employment, both of which are subject to influences from broad governmental actions, such as those related to foreign investment, minimum wage laws, and gender discrimination in the labour market. The Indian study started with child schooling and child labour, both of which are influenced by governmental and private actions, such as the presence of a school in the village, governmental regulations concerning child labour, and opportunities and need for children to work. As such, these two studies shed additional light on the fertility implications of governmental interventions as they operate through female employment in the Philippines and child labour and schooling in India.

Study design

Several types of study designs were considered for measuring the impact of development projects on fertility. Prospective or longitudinal studies, with or without experimental designs, were not initiated because of the time and monetary constraints. Natural experiments were not excluded, but experiments with base-line data on fertility along with information concerning development inputs could not be identified. Studies focused on countries as the unit of analysis to quantify the effects of developmental indicators on fertility were not encouraged because the high level of aggregation in such studies masks important differentials within countries (see, for example, Cutright (1983); and Mauldin and Lapham (1984)).

A cross-sectional approach which relies upon information collected at one point in time was the type of study design used in the five impact studies. This design included the use of stratification. In the studies on the fertility effects of multiple development projects in Sri Lanka and the effects of electrification and irrigation in Thailand, villages were stratified initially on the presence and absence of projects. Villages were then selected from these respective strata. The study in India on child labour and schooling selected villages from districts stratified on the basis of high and low incidences of child labour; and in the Philippine study on the effects of female employment on fertility, areas were stratified on the basis of industrial setting before selecting the samples of individuals.

Level of analysis

Four of the five impact studies collected village-level data and attempted a traditional ecological level of analysis of development impacts upon fertility, with the village as the unit of analysis. However, the results proved unreliable for a number of reasons. First, in cases where village-level data were obtained from an aggregation of the household data, the number of households sampled per village was too small to provide a reliable measure at the village level. Secondly, the number of villages in the samples were too small, resulting in an insufficient number of observations in variable cells for analysis. Thirdly, even in studies where the number of villages could be considered adequate for analysis, the small population sizes of villages further reduced the reliability of the village birth rates based upon these populations. Finally, the village birth rates were also affected by under-registration of births, which is a chronic problem in village registration systems in most developing countries.

A brief example from one of the study data sets will illustrate the effect of these problems upon inferences drawn from an ecological analysis. The results of a multiple regression analysis of village-level data indicated that 20 per cent of the fertility variation among villages was accounted for by selected village-level developmental variables. The village-level availability of irrigation, electrification and health station showed a significant negative relationship with birth rate net of the effect of the other selected variables (see table 1). Moreover, the magnitudes of these effects were higher than that obtained by analysis of the individual-level

Table 1. Multiple regression analysis of average birth rate upon selected development indicators at the village level

Independent variables [a]	Regression coefficient b
1. Length of time irrigation system in the village	-.616*
2. Length of time village has had electricity	-.163*
3. Length of time village has had a health station	-8.549*
4. Utilization of irrigation system	2.342
5. Participation in irrigation system	-1.492
6. Type of village road	1.239
7. Availability of school	-2.494
Constant =	32.82
R^2 =	.20

Source: Jain and Stoeckel (1986).

* = Significant at .01.

a/ Variables 1, 2 and 3 are measured in single years; 4, 6 and 7 are dummy variables; and variable 5 is measured on a 4-point scale.

fertility indicator. A simple listing of the birth rates calculated from the villages for each province in the study by the size of the irrigation system, however, indicated that the conclusions drawn from this analysis would be highly suspect (see table 2). Given that the birth rate in the region from which these data were obtained was around 32, it is quite apparent that the rates reported particularly in the "medium" and "small" categories of irrigation size are substantially underestimated. Admittedly, it could be argued that if these estimates were uniformly underestimated, the results of the regression analysis could be valid. However, this does not appear to be the case.

The above example is intended to illustrate the problems encountered in utilizing ecological analysis in the impact studies. This illustration does not negate the importance of ecological analysis, but highlights the shortcoming of the approach when utilized on data of poor quality and high unreliability.

The level of analysis utilized in the impact studies can be referred to as multi-level. That is, data on variables from a higher level of aggregation were combined with data from a lower level of aggregation, which allowed analyses to be conducted on the relationship between the levels. The two Thai studies analysed the relationship between village-level indicators of irrigation and electrification, their household-level utilization, their household- and individual-level socio-economic characteristics, and fertility behaviour. The Sri Lanka study related household utilization of development outputs to the socio-economic status and fertility of individuals. The remaining two studies related socio-economic status of individuals to individual fertility behaviour, and compared these relationships across strata. All the five studies used appropriate statistical controls to minimize the effect of selectivity bias, which refers to the fact that developmental inputs are not randomly provided to villages. Moreover, individuals benefiting from these inputs are also "self-selected."

Major findings

Three of the five impact studies focused on the impact of interventions to improve economic conditions; one in Sri Lanka and two in Thailand. Findings from these studies are discussed together followed by the findings from the studies in the Philippines and India, which are discussed separately.

Impacts of development projects on fertility in Sri Lanka and Thailand

Interventions to improve economic conditions considered in these studies included land settlement, guaranteed price scheme, rural electrification, and their combinations in Sri Lanka; and rural electrification and agricultural irrigation in Thailand.

In brief, these three studies have established that development projects do affect fertility. They have identified the statistically significant

Table 2. Average birth rate per 1,000 population by size of irrigation project at the village level

Province Number	Size of irrigation			
	Large	Medium	Small	None
1	35.64	13.48	10.08	27.03
2	28.68	(6.76)*	37.37	29.94
3	(6.22)*	20.61	(2.53)*	(17.93)*
4	30.54	26.99	13.24	39.60
5	(7.22)*	(2.84)*	(2.99)*	45.93
6	23.48	(9.92)*	(5.06)*	(59.41)*
7	NS	23.12	(6.15)*	29.47
8	NS	(3.62)*	(9.31)*	23.75
9	NS	(5.70)*	(2.33)*	39.49
10	NS	(15.52)*	60.90	29.28
11	NS	NS	18.74	28.38
12	NS	28.97	19.42	18.35
13	NS	(12.20)*	16.67	31.74
14	NS	(12.81)*	15.32	22.77
15	NS	38.52	13.63	36.70
16	NS	NS	19.90	(15.90)*
Total	20.92 (N=20)	17.42 (N=20)	17.49 (N=20)	31.40 (N=40)

Source: Jain and Stoeckel (1986).

Key: NS = No sample in the province.
N = Number of villages.
* = 3 villages or less.

linkages within this relationship, i.e., the effect of development projects upon socio-economic structure and the effect of the structure upon family planning and fertility, and have shown that the magnitudes of the effects reflecting the former linkages are greater than the magnitudes of the effects reflecting the latter linkages. Further, the overall effect of development outputs upon fertility were found to differ. Interventions to improve economic conditions considered in these studies included land settlement, guaranteed price scheme, rural electrification and their combinations in Sri Lanka; and rural electrification and agricultural irrigation in Thailand. In brief, these three studies have established that development projects do affect fertility. They have identified the statistically significant linkages within this relationship, i.e., the effect of development projects upon socio-economic structure and the effect of the structure upon family planning and fertility, and have shown that the magnitudes of the effects reflecting the former linkages are greater than the magnitudes of the effects reflecting the latter linkages. Further, the overall effect of development outputs upon fertility were found to differ between development projects. The fertility effects of electrification were greater than the effects of irrigation projects in Thailand, and in general the combination of development projects in Sri Lanka exerted greater effects upon fertility than individual development projects (see Sirisena and Stoeckel (1986) for further details of the Sri Lanka study; and Piampiti (1986) and Prasith-rathsint and others (1986) for two Thai studies).

Almost all correlation coefficients measuring the total effects of development projects upon fertility were found to be significant statistically and in the expected direction. In Sri Lanka, except for the land settlement scheme, the average current fertility (during five years prior to interview) for couples who benefited from other projects (rural electrification and the guaranteed price scheme) was lower than that of couples who did not benefit from any of the three development projects. In Thailand current fertility was lower for couples in households with electricity than for couples in households without electricity. The total effect of development projects upon fertility is hypothesized to operate indirectly through the socio-economic structure. This indirect effect of development upon fertility discussed below is the product of its two components: the impact of development projects on the socio-economic structure, such as occupation and income; and the impact of the socio-economic structure on family planning and fertility.

Total indirect effects

With the exception of irrigation projects in Thailand where the fertility effects of the project output at the village level cancelled the fertility effect of the project output at the household level, the total indirect effect of development projects upon fertility are found to be significant and negative, as expected. In Sri Lanka these projects include the guaranteed price scheme alone and in combination with land settlement, and rural electrification in combination with land settlement; and in Thailand rural electrification alone. However, the indirect effects of all these projects, although significant, accounted for a small portion of the total effect of development upon fertility. The reasons are discussed later on.

Effect of development upon socio-economic structure

In general, the research projects in Sri Lanka and Thailand have demonstrated the impact of development projects upon the socio-economic structure at the household level under different conditions existing in these countries (see table 3). The magnitude of these effects, however, was higher in Sri Lanka than it was in Thailand. The effects of the development projects on occupation and income vary by the type of project; the effect of a combination of development outputs on these economic factors was higher than the effect of the individual output; and these effects were gender-specific.

All projects in Sri Lanka made a greater contribution to increasing male income than they did to increasing female income net of their education and occupation. The link between occupation and income among females was much stronger than among males. An internal constraint affecting the level of female income in the agricultural sector was that their wage rate for the same occupation was lower than that for males. These gender-specific impacts of development projects are the first to be empirically demonstrated in Asia.

Socio-economic structure and fertility

Although development projects exerted effects upon occupation and income, the effects of these socio-economic factors upon family planning and fertility were mixed. Moreover, the magnitude of these effects was lower than that of the effects of development projects on occupation and income. For example, in Sri Lanka, participation of females in occupations outside the household does not necessarily result in a positive effect upon the practice of family planning. It is the type of employment such as participation in "public sector" occupations, that is important for the increased use of contraceptives. All development projects, while increasing participation of females in occupations outside the households, did not increase their participation in public sector occupations. However, female income appears to be more important than male income with regard to its effect on the use of contraceptives. These results suggest that those development projects that contribute to an increase in participation of females in public sector occupations, or to an increase in female income, would also contribute to lower fertility.

Impacts of female employment upon fertility in the Philippines

The Philippine study tested the applicability of the "role incompatibility" theory, which maintains that the employment/fertility relationship, should be weakest in cases where either occupational or household factors operate to reduce conflicts between working and maternal roles. It compared women in three types of urban work settings with housewives. The occupational settings included a large-scale canning factory, smaller sized shops and industries, and vendors in one of the city's four public markets. Overall the findings failed to support the role incompatibility theory.

Table 3. Effects of development programmes on occupation and income in Sri Lanka and Thailand

Development programme	Occupation				Income					
	Non-agricultural male		Non-household female		Male		Female		Household	
	r	B	r	B	r	B	r	B	r	B
Sri Lanka: [a]										
1. Guaranteed price scheme	-.38	-.38*	.44	.44*	.42	.39*	.35	.01*		
2. Land settlement	-.47	-.47*	.29	.28*	.32	.32*	.29	.04*		
3. Rural electrification	-.34	-.28*	-.02	.04	.39	.21*	.15	.13*		
4. 1 and 3	-.05	-.08*	.01	.06*	.70	.63*	.19	.15*		
5. 1 and 2	-.47	-.47*	.43	.41*	.25	.24*	.40	.06*		
6. 2 and 3	-.31	-.35*	.44	.46*	.70	.62*	.43	.13*		
Thailand:										
1. Electrification [b]	.26	-.08**	.24	.08**					.13	.01
2. Irrigation [c]	.07	-.04**	.06	.05**					.06	.10**

Source: Jain and Stoeckel, 1986.

r: Observed correlation coefficient. It measures the total effect of a particular programme.
B: Standardized regression coefficients show the net effect of a programme.
* $p<.01$
** $p<.05$

a/ Effect of male (or female) education controlled for male (or female) occupation; effects of both education and occupation controlled for income.

b/ Effects of village availability of health station and irrigation, and household use of electricity and wife's education, controlled for both male and female occupations; effects of these variables and occupation of both male and female controlled for household income.

c/ Effects of length of time the irrigation project has been in the village, village availability of electrification, health station and school; and effects of size of landholding, female education and participation in the irrigation project controlled for both the occupation variables. Effect of these variables along with the occupation of the spouse controlled for household income.

Although fertility differentials were found among the three occupational groups they were not in the direction hypothesized by the theory. Current levels of fertility were highest among factory employees and lowest among housewives.

The Philippine study identified a major oversight of the role incompatibility theory: that is, on the one hand it can create pressures towards the reduction of natality due to its hypothetically positive association with family planning use, but it can also increase the fertility of working women through its restrictive impact upon the practice of breast-feeding. Hence, in settings where breast-feeding levels are an important factor in regulating fertility levels, either because contraception is not widely practiced or (in the Philippine case) because the less effective methods are commonly employed even if use is fairly widespread, it is to be doubted if incompatibility between maternal and occupational roles will result in decreased fertility. This would be particularly true in situations (again, as exemplified by the Philippine case) in which breast milk substitutes are readily available, thus making viable the option of deciding not to breast-feed one's baby (see Costello and Palabrica-Costello (1986), for further details).

Impacts of child labour and schooling on fertility in South India

The study in Karnataka State, in India, first established the nature and the extent of work performed by children in rural households, established a negative relationship between child labour and schooling, identified some of the important determinants of both child labour and schooling, and then studied their impacts on family planning and fertility.

On an average children in rural Karnataka spend four hours per day on work (household work and directly productive work). Their participation increased with age as expected. Sex differentials were found both in the nature of work - greater household work by girls and greater directly productive work by boys - and in the time spent on work: girls put in longer hours than boys. Relatively fewer girls than boys went to school. A significant inverse relationship between child labour and schooling was found at the micro-level. The age of the child and the presence of younger children in the household were important predictors of child labour; and age of the child and education of the father were the important predictors for child schooling.

Although no direct relationship was found between child labour and fertility, child labour did significantly affect fertility indirectly through its negative effect upon schooling. Since schooling negatively affected fertility, the overall effect of child labour on fertility was found to be positive. This relationship persisted net of the significant negative effects upon fertility of father's opportunity consciousness and the number of living children. Further, socio-economic factors, such as landholding and father's education, proved insignificant in the relationship. Hence, it would appear

that both reductions in the incidence of child labour and increases in the proportions attending school and attaining higher levels of schooling will contribute to a reduction in fertility (see Kanbargi and Kulkarni (1986) for further details).

Methodological issues

Development-related public interventions are designed and implemented at aggregate levels usually defined by territorial boundaries. Fertility behaviour, on the other hand, refers to the behaviour of individual couples. The rationale for expecting the effect of macro-level interventions on the micro-level behaviour flows from the assumption that "social context or social normative variables must affect fertility" (Freedman, 1985, p. 269). Inputs provided through development projects influence the social context within which decisions about family size are made by individual couples. Specification and measurement of these linkages are essential ingredients in assessing the impact of a development project on fertility. Whether to link the macro-level interventions with the micro-level behaviour, and how to link them, thus are some of the major issues in research related to the measurement of the impact of development projects on fertility.

Research on fertility impacts of development, as in other areas of social science, requires models or explanatory frameworks which can adequately describe the nature and direction of relationships between variables. The complexity of the interrelationships between fertility and development makes this a formidable task, particularly since developmental outputs must operate indirectly through a host of factors to affect fertility. The researchers faced a number of issues in undertaking the impact studies reviewed here. These included control, selectivity, simultaneity, multi-colinearity, time lags and incompleteness. These issues were addressed at the analysis stage by using, for example, appropriate statistical controls, and at the design stage by identifying development projects that were implemented a number of years prior to the study. These issues are not discussed in this paper. The reader is referred to the individual studies and to the chapter on methodological issues in the volume edited by Stoeckel and Jain (1986). In this paper, we focus on three issues: how to measure the impact of development on fertility; how to evaluate the strength of this relationship; and what should be an appropriate level of analysis.

Measurement issues

The basic issue in an impact study is how to measure this impact. The impact of a development project on fertility can be measured by its total effect and by the interaction effect. The total effect, however, will overestimate the impact of development, because it is made up of four components: (a) spurious effect due to selectivity biases; (b) indirect effect explained by the model; (c) indirect effect not explained by the model, i.e., its direct effect; and (d) residual random term. The impact of a development project can thus be measured by direct, indirect and interaction effects.

Direct effect. Should development project output show any direct effect upon fertility? Working within the conceptual framework described earlier, it would be unrealistic to expect any direct effect of development project outputs on fertility, if the model was adequately specified. In any empirical analysis, even the household- or individual-level socio-economic factors should not show any effect on actual fertility after controlling for the relevant intermediate or proximate variables. Any direct effect of, for example, female education on a woman's fertility would simply imply that either all the proximate factors are not included in the model or they were not properly measured. If this assertion is accepted, then it follows that macro-interventions in terms of development projects cannot show any direct effect on indicators of actual fertility after controlling for the effects of the household- and individual-level socio-economic characteristics and the proximate variables.

Can the development projects have independent effects on the measures of demand for children? Assuming that the demand for children is adequately measured, it is conceivable that development project outputs can affect the demand for children by changing childbearing incentives or by creating a demand for consumer goods and social services. For example, the presence of a school in the village, by keeping children in schools, may, contribute to a decrease in the utility of children by reducing their labour inputs. This in turn may reduce the demand for children without changing the parents' income. Other infrastructural facilities, like rural electrification can create demand for consumer goods which in turn may create a demand to reduce family size. Most of the effect of development outputs on childbearing incentives, however, would result from changes in economic and social structures. Without such changes, for example, high demand for child labour may result in high dropout rates from schools rather than a desire for smaller families. Thus, the development project outputs are unlikely to substantially reduce the desired family size directly, i.e., independent of the effects transmitted through changes in economic and social structures.

Interaction effect. Does the presence of a development project (e.g., rural electrification) at the community level affect the relationship between individual characteristics (income or occupation) and fertility? This question can be addressed by including an interaction term between a development project (e.g., electrification) and individual characteristics (e.g., income) in the analysis (see Hermalin, 1985).

A slightly different approach was used in the Sri Lanka study, which compared six régimes of development projects in terms of their net effects on occupation and income, the net effects of occupation and income on the use of family planning methods, and the net effect of family planning on recent fertility. Since couples living in areas without any of these project inputs were used as reference, these comparisons can give a good indication of the extent to which the presence of a development project modifies the relationship between individual level characteristics. The results showed that the relationship between the use of family planning methods and fertility was not modified by the development projects under investigation. Also, the

projects did not modify the effect of (male or female) education on occupation, income and the use of family planning methods. However, the effect of occupation on income as well as the effects of these two factors on the use of family planning methods were not the same for each of the six régimes of development projects, whether one considered male characteristics or female characteristics. Since the six development project régimes considered by the Sri Lanka study affected occupation and income of both males and females, these projects also modified the relationships between occupation and income and their effects on the use of family planning methods. Since these projects did not affect education, they, perhaps, do not modify the relationship between education and the use of family planning methods.

The presence or absence of an interaction effect could not be ascertained in the two Thai studies. These effects, if any, were implicitly included in estimating the indirect effects of project outputs on fertility, because villages and households that did not benefit from the project outputs were included in the analysis for estimating the net effects of individual characteristics on the desired family size and on the proximate determinants included in the models.

Evaluation of the effect of development projects on fertility. How should the strength of the relationship between project output and fertility be evaluated? This question has two dimensions: how to evaluate the effect of a particular project, i.e., does rural electrification affect fertility? and how to assess the relative effects of different projects across studies.

The second question can be addressed appropriately only if the two studies are comparable in terms of the conceptual framework, study designs, model specifications and measurements. The total effects of development projects on fertility and the use of family planning, for example, in Thailand turned out to be smaller than in Sri Lanka, because these studies were not comparable in one important aspect. In the Sri Lanka study, the beneficiaries of a development project were compared with the couples living in areas without any development project output. In the two Thai studies, both the beneficiaries and non-beneficiaries living in rural areas with irrigation (or electrification) were compared with couples living in areas without irrigation (or electrification). The effect on non-beneficiaries and, therefore, the effect on total population living in areas with a development project naturally will be smaller than the effect on beneficiaries alone.

In empirical analysis, one way to evaluate the strength of a relationship is to assess whether or not it is statistically significant. This was done for the total effects, as well as for the indirect effects of development projects on fertility explained by the model. However, the magnitude of these effects turned out to be small. Moreover, the decomposition of the total effect into four components - spurious effect, indirect effect explained by the model, and two residual terms - for the Sri Lanka study, showed that in comparison to the indirect effects explained by the model, the magnitudes of the two residual terms were quite substantial (see table 4). The first residual term is associated with the measurement of family planning

Table 4. Decomposition of the total effect of development projects on fertility in Sri Lanka

Development programme	Total (1)	Indirect[a/] (2)	Spurious[b/] (3)	Residual I[c/] (4)	Residual II (5)
1. Guaranteed price scheme	-.124	-.0045	-.0391	-.0019	-.0785
2. Land settlement	.024	-.0059	-.0189	.0108	.0380
3. Rural electrification	-.103	.0031	.0319	-.0001	-.1380
4. 1 and 3	-.153	-.0020	.0079	-.0304	-.1285
5. 1 and 2	-.016	-.0073	.0070	-.0171	.0014
6. 2 and 3	-.086	-.0061	-.0178	-.0153	-.0468

Source: Jain and Stoeckel, 1986.

a/ Explained by the model through occupation and income of wife and the use of family planning.

b/ Due to association of project with duration of marriage and female education.

c/ Refers to lack of specification of indirect determinants of fertility and measurement errors associated with the use of family planning.

d/ Refers to lack of specification of proximate determinants of fertility and measurement errors associated with the indicator of fertility.

and lack of specification of indirect determinants, and the second term is associated with the measurement of fertility and lack of specification of proximate determinants.

The main explanation for getting such a large residual category and small indirect effect involved the measurement of family planning use and fertility rather than the lack of specification (see Jain and Stoeckel (1986) for elaboration of this point). The fertility measure is based on the number of births taking place during five years prior to interview. Such a measure, by definition, will be affected considerably by the stochastic nature of the event - birth. In fact, the percentage of variance explained in this measure of fertility by a host of proximate and indirect determinants and their regression coefficients are rarely high. The use of family planning method referred to the current use, and was measured by a dummy variable. A better measure of use incorporating its duration would, perhaps, increase the path coefficient from family planning to fertility. It would also increase the magnitude of the path coefficients from occupation/income to family planning use. These increases would raise the magnitudes of indirect effects explained by the model and would lower the magnitudes of both the residual terms. Unfortunately, none of the studies collected data on the duration of family planning use.

In addition to measuring the statistical significance, the relative importance of a particular development project can also be ascertained by comparing its total effect with the effect of any individual or household factor or the use of contraception on fertility. The results, shown in table 5, indicate that in terms of the absolute magnitude, the programme effect on the use of contraception compares favourably with the effects of individual factors in all cases except for two projects: land settlement and guaranteed price scheme treated separately.

In terms of the effect of development project output on recent fertility, the absolute magnitudes compare quite favourably with the effects of the individual-level factors and even with the effect of the use of contraception. These comparisons do not imply that the development projects considered here are as important as the use of contraception in reducing fertility, but they certainly provide a reason to pause before concluding that the effect of development projects on fertility is small or irrelevant or disappointing. This type of conclusion was reached in a number of studies that focused upon the estimation of the direct effect, or independent contribution, of community-level variables. The absence of a significant independent or direct effect of community variables (e.g., rural electrification or village school) on fertility in these studies was erroneously interpreted as indicating no effect of community variables on individual fertility behaviour. Instead of direct effect, these studies should have focused upon the mechanisms through which community-level variables affect fertility.

The actual relevance of development projects can perhaps be judged only in terms of their impacts on social and economic structures. The results presented earlier in table 1 show that these effects without a doubt are quite substantial in all cases.

Table 5. Comparison of the effect of development programmes with the effects of other variables on the use of family planning and recent fertility in Sri Lanka and Thailand

Development programme	Correlation coefficients: Use of contraception: Project output (1)	Use of contraception: Average (2)	Recent fertility: Project output (3)	Recent fertility: Contraceptive use (4)	Recent fertility: Average (5)
Sri Lanka:					
1. Guaranteed price scheme	.04	.12	-.12	-.19	.14
2. Land settlement	-.02	.12	.02	-.23	.10
3. Rural electrification	-.08	.16	-.10	-.23	.07
4. 1 and 3	.17	.13	-.02	-.21	.13
5. 1 and 2	.17	.10	-.15	-.25	.11
6. 2 and 3	.14	.11	-.09	-.23	.13
Thailand:					
Electrification	.08	.07	-.07	-.01	.03
Irrigation	.04	.03	-.05	-.05	.03

Source: Jain and Stoeckel, 1986.

Note: Columns (2) and (5) show averages of correlation coefficients without considering their signs. In the Sri Lanka study, six characteristics for which averages are calculated include education, occupation, and income of male and female separately. In the Thai study of electrification, education and occupation of males and females and household income are included in calculating averages. In the Thai study of irrigation, wife's education, occupation of male and female, and household income are included for these averages.

Appropriate level of analysis

Should the macro-level intervention be linked with the micro-level reproductive behaviour or should it be linked with the macro-level indicator of fertility? The answer, of course, would depend upon the purpose of the study. If the interest is in the understanding of the mechanisms through which the macro-level intervention can affect micro-level fertility behaviour, then the multi-level analysis approach is appropriate. But if the interest is in differentiating communities then the traditional ecological analyses would be more appropriate (see, for example, Cutright (1983) for countries as the unit of analysis; Hermalin (1975) for smaller administrative areas in Taiwan as the unit of analysis; and Jain (1985) for states within India as the unit of analysis).

The ecological analysis would also be appropriate to address one of the 40 questions listed by Bulatao and Lee (1983) on their agenda for research: what variations in institutional and community settings are consistently and reliably related to higher or lower fertility? In their expansion of this question, they suggest: "It is also advisable, however, to have some research that begins at the other end, not with individual or households models, but with types of institutional and community settings, distinguishing and cataloguing them and determining which are linked to differences in fertility". Bulatao and Lee did not specify whether they were referring to fertility differences among individuals or among communities. If all individuals exposed to one type of community setting are equally affected by it then it is immaterial whether the reference is in the explanation of fertility differences among individuals or among communities. Both will be the same. However, it is rarely the case. Under most circumstances, all individuals exposed to an institutional setting are not equally affected. Hence, the type of question quoted above can be addressed only by linking macro-level factors with the aggregated index of fertility.

Problems encountered with ecological analysis utilizing data of poor quality and unreliability have been illustrated earlier. Some of these problems can be minimized by increasing the sample size, i.e., the number of villages as well as the number of households per village, which of course will increase the cost of data collections. Other problems associated with the deficiencies of vital registration systems cannot be resolved in the absence of complete enumeration of the villages included in the study. Even with the availability of good quality and reliable data, one has to be concerned with the possibility of drawing spurious conclusions from ecological analysis. For example, in an ecological analysis of all developed and developing countries, the correlation between the indicators of breast-feeding and fertility would perhaps turn out to be positive. If so, it cannot be interpreted literally to mean that breast-feeding increases fertility, because from individual-level studies it is known that breast-feeding suppresses fertility. This type of situation is commonly called an "ecological fallacy." Thus, in the absence of some knowledge about the way a development project is supposed to influence fertility behaviour of individuals, the ecological analysis could be misleading.

The use of individual-level or multi-level analysis, on the other hand, can be discouraging because of the low magnitudes of beta coefficients of or the percentage variance explained by factors included in the analysis. This has been demonstrated by impact studies discussed in this paper. It seems to us that the best solution lies in the combination of the two approaches. One should first use the individual or multi-level approaches to understand the mechanisms through which micro- or macro-level factors affect micro-level fertility behaviour. One should then use ecological analysis to identify a subset of these micro- or macro-level factors that are most important in explaining fertility differences among communities or societies.

Implications for further research

The purpose of the fertility impact studies is to quantify the actual fertility changes (differences) attributable to inputs provided through development projects. These research efforts are basically geared to identify points of interventions that will affect demographic processes in the long run. Each link in the chain of events between the initiation of a development project and its ultimate effect on fertility requires time: for the initial study to measure the effect of development projects on fertility; for translating and disseminating the policy relevance of the research findings to policy makers; for changes in the allocation of resources to take place, assuming that fertility impacts are taken into consideration; and for the implementation of these policy changes to have some measurable impact on fertility. A quick glance will be sufficient to indicate that this is a long-term process. These issues of time lags must be clearly understood, so that expectations are not raised regarding short-term solutions. Since the population problems are not short-term in nature, these types of research efforts are important because they are most likely to have a significant effect on the demographic processes in the long run (see Jain (1978) for further discussion of the utility of research on the demographic impacts of development).

The results of impact studies are unlikely to be used to reject a development project because most of these projects can be adequately justified on the basis of their primary objective alone. However, the outcome of impact studies can generate guidelines for assigning higher priority to some development projects on the basis of their anticipated anti-natalistic effects and, more important, they can be useful in improving the design of a particular project in order to enhance its antinatalistic effect.

In order to enhance the utilization of these activities, researchers should understand the process of decision-making, and to identify various elements and their relative importance that are considered by policy makers in selecting a particular strategy. This can best be done by establishing active collaboration between researchers and those responsible for selecting, designing and implementing development programmes at various stages, from the formulation of a research problem to the implementation of its findings.

The utility of fertility impact studies can be further enhanced by monitoring changes associated with new development projects from the beginning, rather than by trying to estimate change attributable to these projects after their completion. The projects for this purpose, however, should be carefully selected. First priority should be assigned to projects that are (a) organized to directly affect people in the project area; (b) designed to influence those social, economic and cultural conditions that are believed to affect fertility by reducing childbearing incentives and increasing the cost of childbearing; (c) organized at a sub-national, well-defined, geographic area with a population of about 50,000; and (d) provided with some evaluation plan to study the effect of project outputs on the socio-economic structure in the project area.

Although prospective studies are expensive and difficult to carry out, such studies can be much more useful than the cross-sectional type of studies. Their outcome be further improved if they are conducted in areas with a good vital registration system.

In areas with poor registration systems, good surrogate indexes should be developed to measure the impact of development projects on fertility. These indexes might include such factors as costs and benefits of children, familial institutions, and preferred or desired family size. Other indexes may include key factors such as infant and child mortality, child labour and schooling, female education and women's income and employment, which are usually found to be associated with a demand for smaller families and an increase in the use of contraception. Research on project-specific impact on and determinants of these known or presumed key determinants of fertility would help to identify ways to modify them through development policies and programmes in a way that potential changes in these determinants would be consistent with demographic objectives.

Implications for population policy

The fertility impact studies reviewed in this paper have made important contributions to the knowledge base concerning the quantitative impact of development projects on fertility. The studies are unique because they have not only assessed the impact of development projects on fertility by simple gross differences but also have specified and estimated linkages through which development projects affect fertility. They reveal that development projects affect fertility significantly through influencing socio-economic structure, which in turn affects the use of family planning methods. These indirect effects were found to vary between development projects. Electrification exerted a greater impact upon fertility than irrigation in Thailand, and combinations of projects exerted greater impacts than individual projects in Sri Lanka. Further, the studies established empirically the gender-specific impacts of development projects upon occupation and income, and the significant indirect effects of child labour upon fertility as it operates through a decrease in time spent by children in schools in India. The

Philippine study identified a major oversight of the role incompatibility theory by documenting different mechanisms through which occupational settings can influence fertility. The Indian study confirmed the expected inverse relationship between child schooling and parental fertility and showed that the effect of child labour on parental fertility is transmitted though its effect on child schooling.

The following broad implications for population-related development policy can be drawn from these findings.

(1) Combinations of development inputs (e.g., electrification, irrigation, price supports) are more likely to have a cumulative fertility impact than single interventions, and are most likely to have an impact where family planning services are available and accessible, but the land settlement schemes without other support systems are likely to increase fertility.

(2) The type of female employment generated by development projects is most important, with non-agricultural and public sector employment having the greatest impact on depressing fertility.

(3) Raising female income through equalizing wage rates within existing employment structures could have a fertility depressing effect, as suggested by the Sri Lanka study.

(4) The Philippine study found that reduced opportunities for breast-feeding by mothers working in factories accounted for the absence of expected fertility differentials between these women and housewives. Hence, the need for policies in support of government or factory-provided child care facilities which would support and make possible extended periods of breast-feeding by working mothers.

(5) Policies directed at reducing the need for child labour and increasing school enrolments would have fertility depressing effects.

(6) As a corollary, policies to reduce school drop-out rates, especially of females, could be expected to have the maximum antinatalist effect.

(7) Information about contraceptive practice should be incorporated into development project design, by including current contraceptive prevalence as one criterion in selecting development project sites, and by ensuring that contraceptive services and information are subsequently made available in project areas.

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B. Methodological issues in assessing the population impact of rural development projects

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Rural development projects usually have as their goals an increase in agricultural production, improvement of the quality of life in rural areas or the settlement of previously unoccupied lands for social, economic, or political reasons. Many rural projects are designed to meet a combination of goals and some have demographic goals as well, notably slowing rural-urban migration. Except for the necessity of migration for the settlement of new lands, however, project planners have given little thought to the demographic aspects and impacts of their projects. Moreover, there have been no "ideal" studies from which a researcher may clearly discern before-and-after project patterns; in fact, there have been few studies of any kind evaluating the demographic effects of development projects.

In the first part of this paper, five pilot field studies suggesting some guidelines about how to design and carry out population impact studies are briefly reviewed. Nevertheless, this methodological assessment does not recommend a universally "correct" approach based on household survey data collection (the focus in four of the five case studies). This point is emphasized in the second part of this paper, which reviews a more recent (and on-going) effort to collect data at the project area or community level to assess the demographic effects of integrated rural development projects in Ecuador.

The rather unusual and attractive institutional context of the five case studies is described. These studies comprise the most relevant part of the Carolina Population Center experience, cited in the conference programme. 1/ The methodological assessment of the studies attempts to point the direction towards improved approaches in the future. It begins with a brief summary of project purposes and findings before comparing the theoretical approaches, data collection procedures, analytical methods and policy implications. (Further details are found in Bilsborrow and DeLargy (1985) especially in chapter 7.)

The second part of the paper describes a very recent - indeed, an on-going, and incomplete - experience in collecting areal-level data from government administrative and census statistics, project authority files, and community level interviews in the field to assess the demographic effects of integrated rural development projects in Ecuador. Some of the problems encountered and approaches adopted are briefly mentioned.

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Experience of the International Consortium of Research Centers on Population and Development

The International Consortium of Research Centers for the study of the impact of development projects on population was initiated in the spring of 1979 by the Carolina Population Center (CPC) of the University of North Carolina, the Office of Policy Analysis and Statistics and Planning Section of the United Nations Fund for Population Activities (UNFPA) and the Population Division of the Rockefeller Foundation, to study the benefits of international co-operation among population research centres focusing on a common research concern: specifically, how to evaluate the impact on population of development projects whose primary goals, typically are not related to population.

The Consortium consisted of six member institutions, with CPC serving as secretariat and headquarters for co-ordination and data analysis. Each of the other five institutions was represented by a principal investigator who developed and conducted a study on the demographic impacts of a specific development project in his or her country (see annex). These projects were all of quite modest size and funded by UNFPA or the Rockefeller Foundation. Most were initially selected by those agencies and not by CPC.

Mechanisms through which Consortium members learned from each other included a preliminary workshop in September 1979 to attempt to achieve some commonalities in the design of projects; subsequent circulation of reports during the field-work period; a follow-up workshop in June 1981 at which principal investigators discussed problems encountered, analytic methods used, and preliminary findings; and finally the preparation of the edited book for UNFPA by CPC to disseminate the results.

The experience of the International Consortium convinced CPC of the value of university-to-university institutionalized, funded collaborative research. An annex in the Bilsborrow and DeLargy (1985) describes the experience, the problems encountered and lessons learned.

Summary and comparison of substantive findings

The five studies were undertaken beginning in 1979 or 1980, at least partly to assess the demographic effects of rural development projects. Two studies assessed the effects of rural electrification, two the effects of colonization/settlement schemes, and one the effects of an irrigation programme. Each was based on primary data collection at the household and/or community level. Data for a control group population were also collected in each study, though the control groups were quite limited in three of the five studies.

Topics analysed included effects on in- and out-migration, fertility, knowledge and use of family planning methods, mortality and morbidity, and a host of "intervening" variables such as household production, women's

employment, problems of adaptation in the new area, school attendance, health problems, and plans for staying or not. Most studies were carried out on small budgets.

The two electrification projects (of Herrin in the Philippines and ter-Wengel in Colombia) found a wide range of positive economic changes made possible by the availability of electricity, including increased output and increased consumption of electricity-using consumer durables. But from that point the two studies diverge, the former looking only at fertility and the latter only at migration. By comparing rates of out-migration from two villages over time, the author of the Colombia study concluded that village electrification resulted in a decline in the rate of out-migration. But in fact the results are not statistically significant. The Colombia study used aggregate (aggregating across households) time-series analysis in contrast to the Philippine study, which was based on micro-, cross-sectional analysis. The Philippine investigation is, indeed, the only study among the five to investigate the effects of household and community variables together on household behaviour. Both individual and community use of electricity were found positively associated with a woman's use of modern family planning. Household fertility was found to be negatively influenced by electrification of the area but not by electrification of the particular household. This anomalous finding may partly result from an inadequate conceptualization of community-level effects (see Bilsborrow and Guilkey, 1987).

The irrigation project, investigated by Pramote Prasartkul and colleagues in Thailand, found that substantial positive socio-economic and demographic changes occurred as a result of irrigation. Economic changes included increased multiple cropping, more regular employment, and higher levels of production. While the resulting increase in land values was thought to be responsible for lack of in-migration to the irrigated villages, the other hypothesized demographic effects did seem to occur, viz., improved health, increases in the perceived cost of children, and, most clearly, declines in fertility and increases in use of family planning.

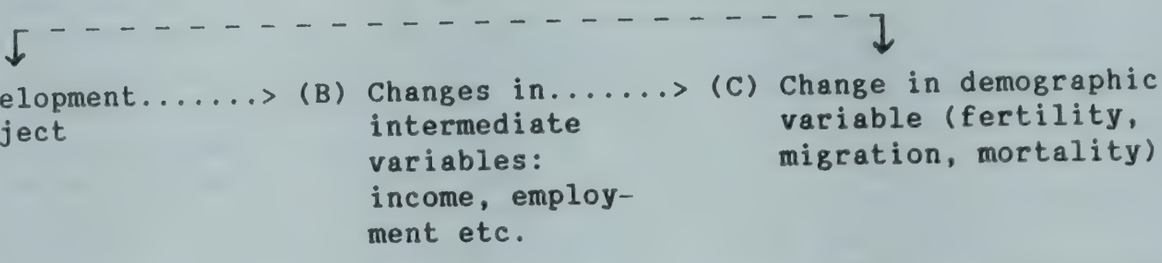
The two colonization or land settlement projects (studied by Uyanga in Nigeria and Henriques in Brazil) are similar in that both were instituted by the federal Government to attract people to land-plentiful areas from people-plentiful areas. Both achieved this goal. Moreover, those migrants who actually became legal settlers, receiving wages for their labour on plantations in the case of Nigeria, and titles to large plots of land in the case of Brazil, intended to remain. Large numbers of spontaneous settlers came to both areas as well, which is not unusual in such projects. But in Brazil, the heavy influx of migrants seriously diminished the possibilities for improvements in living conditions of migrants, including health and mortality. In Nigeria, even though schools and health clinics (with family planning methods available) were much more accessible to the plantation than the non-plantation control populations, levels of fertility and family planning use remained about the same to date. However, differences in the school attendance of girls and in attitudes towards family planning do suggest some prospects for future differences to evolve but only in the long run. In Brazil, fertility levels also remain very high, and there are no explicit goals to reduce fertility or increase family planning.

Research design: problems encountered and lessons learned

The research methods used in the five case studies are briefly compared, identifying where problems were encountered and where improvements, within the scope of the very limited financial resources available, might have been possible. Theoretical aspects are discussed first, followed by data collection and data analysis.

Theoretical and modeling considerations

The theoretical approach followed by all investigators may be indicated simply as follows:



That is, the development projects were undertaken to improve the living conditions of the population in the country rather than to alter demographic processes. This does not mean the projects were not expected to have demographic effects, but rather that any such effects were incidental. This is the usual situation with economic development projects, though in some projects it may be possible to incorporate features in the original project design to maximize favourable anticipated demographic impacts or to alter unfavourable effects. In a 1979 workshop, World Bank officials considered this possibility but expressed reservations because of the technical complexities of the process (King, 1979).

The projects investigated in the Consortium were economic projects, intended to have effects on production, employment and consumption. Their demographic effects are only indirect, as seen in the diagram paths from A to C; thus there are no direct effects of rural electrification or irrigation on C. The dotted line designates effects that exist for some other types of development projects, such as the expansion of family planning and health facilities. In those instances, any economic effects would be indirect (i.e., the path would be A--->C--->B).

Of course, the International Consortium did include projects of the A--->C type if one is considering the effects on migration. The projects in Nigeria and Brazil are colonization projects, which are explicitly intended to alter population distribution by changing migration patterns. They essentially do this, however, by altering the physical environment and infrastructure in an area so as to improve economic opportunities. The path is A--->C--->B, since people have to be attracted and actually move to the

area before production levels or other forms of behaviour can change. After they arrive, production can increase. In Nigeria, government plantations were created to attract wage workers and their families. Substantial social infrastructure was also provided, (e.g., schools and health clinics) which may directly influence the fertility and mortality of in-migrants.

Unfortunately, there is much more to consider in appraising the demographic effects of development projects than a path diagram, A--->B--->C. Consider fertility. Ideally, the extensive literature on the determinants of fertility should be drawn upon to develop a model that incorporates the role a particular development project has in influencing fertility. Theoretical approaches include economic theories of household decision-making, socio-psychological approaches, and "supply" focused models emphasizing the role of family planning. Approaches that integrate elements from all seem superior a priori (see, for example, Easterlin in Bulatao and Lee, 1983), but the practical problems of combining all into an estimable model make simplifications necessary and add to the appeal of appropriately augmenting economic models (see Bilsborrow, 1981; Schultz, 1981; Bulatao and Lee, 1983; Farooq and Simmons, 1984; Bilsborrow and Guilkey, 1987). In fact, none of the cases evaluated demographic effects using an explicit theoretical model that would permit the assessment of project effects on fertility or migration.

To further complicate assessment, the economic development project is often part of a programme of changes in infrastructure. Other components of the programme may include education and health policies, which usually have more direct demographic effects. But then any observed changes in fertility or mortality cannot be attributed to the economic development project but rather to other components as well. In the projects investigated, the development project was accompanied by other concomitant changes in infrastructure, which greatly confounded the estimation of the A--->C effects for a particular project since it was instituted pari passu with other projects. And since any of these projects alters the environment in ways that may influence villagers' lives, these changes should all be taken into consideration and evaluated simultaneously.

To illustrate, considerable infrastructure was provided as part of the plantation settlement programme in Nigeria, so that "programme effects" observed refer to the effects of that particular package of infrastructure. In Thailand, adjoining the new irrigation canal was a new road connecting the villages in the project area to important small cities in the region; in addition, national family planning and rural health programmes were being expanded and may have been differentially administered in the villages selected. Such national programmes also accompanied the regional electrification programme in the Philippines.

Data collection

Survey and sample design. The survey design should develop out of the theoretical framework. In principle, a careful assessment of the effects of a development project requires that: (a) the project be implemented in a

large, randomly selected number of areas; (b) there be no other concurrent development projects in the areas; and (c) both baseline and subsequent instruments be used to collect detailed information on households affected, to compare them "before" and "after". These are aspects of an appropriate "before-after, two-group" experimental design of the survey. If (a) is true, areas not receiving the project may be compared with those receiving it ex post, and serve as control areas. If (a) is true, then even if (b) is not, the problem is at least mitigated. If the other projects are both randomly distributed and have smaller effects on the criterion variable(s) than the project under investigation, the separate effects of the project will be discernible but more difficult to quantify. Unfortunately, in practice (a) is probably never true, for reasons of budgetary restrictions or political realities (persuasive politicians and well-organized pressure groups who get more projects implemented in their areas). And (c) has not been done in the past for the necessary demographic and related data, though, with foresight, this need not continue to be the case.

The approach of the five studies in the International Consortium was to conduct a single round survey in areas that had the prior development projects and in areas that did not. The form of analysis implied was thus cross-sectional, comparing "treatment" (in biostatistical terminology) and control populations. Thus, one is interested in whether $X_t(P) > X_t(C)$ for output or "effect" variable X (e.g., income, reduction in fertility etc.), where P refers to population in the project area, C to population in a control (non-project) area, and the subscript t designates time (e.g., time of interview).

The utility of cross-sectional analysis for evaluating the impact of a project is based on the assumption that changes across households at a point in time reflect changes over time within households. Economists often claim that cross-sectional differences reflect long-run or "equilibrium" conditions. But in practice sampling limitations - almost inevitable - are likely to be such as to raise questions about the comparability of areas. One alternative ((c) above) is to assess the effect of the project on a specific population over time. That is, one may compare $X_t(P)$ with $X_o(P)$, where the population received the development project between time 0 and time t. A single retrospective household survey may be able to collect adequate information from households on their situations at both times 0 and t, although a panel survey is preferable.

Table 1 shows the data collection approaches in the five projects. Four of the five used household surveys, and two, community surveys. All five collected data for control populations, though this was very limited for the two in Latin America. The use of only one control population in these two areas (and only two in Thailand) evidently greatly weakens the validity of any conclusions about project effects from cross-sectional comparisons. To the credit of all three authors, their analysis plans attempted to get around this by also comparing the populations over time (point (c) above, see below).

The two projects with more adequate control populations were those of the Philippines and Nigeria. But the numbers in table 1 are misleading. In Nigeria, the 200 control villages were selected from an area 100 to 200 miles

Table 1. Methodological comparison of projects: data collection

	Philippines	Colombia	Thailand	Nigeria	Brazil
Type of project	Electrification	Electrification and school extension	Irrigation	Plantation settlement	Colonization settlement
Type of data collection	Household and community survey	Household survey	Community survey	Household survey	Household survey
Number of sample points (communities or areas)	20	2	5	208 a/	2
Sample size (households)	800	200	508	4036	800
Use of control group	Yes	Yes, indirect	Yes	Yes	Yes, indirect
Duration of training of interviewers	3 days	n.g.	1 month	n.g.	2 weeks
Ratio of interviewers to field supervisors	n.g.	n.g.	n.a.	10:1	6:1
Interviewer characteristics	Residents	Residents	Non-resident students	Mostly residents	Non-residents
Interviewee	Head and wife	Head	Selected residents	Head and wife	Head and wife

Source: Bilsborrow and DeLargy (1985), pp. 178-179.

a/ 8 plantations and 200 control villages.

n.a., not applicable.
n.g., not given.

away from the project to ensure no contamination, but in fact they were still areas of some out-migration to the plantation projects. The control areas were also allocated along several main roads to simplify interviewer access. A similar judgemental selection procedure was used in the Philippines, with the treatment and control areas statistically compared ex post: "Detailed community data...were obtained...to test the assumption of homogeneity of the eastern and western barangays" (Herrin and ter-Wengel, in Bilsborrow and DeLargy, 1985). While the formal statistical test implied homogeneity, there were differences whose effects are indeterminant.

We now consider the procedures used for selecting households within areas, which is not relevant to either Thailand (a community survey) or Colombia (where all households in the two towns were covered). Household sample sizes varied widely, from 200 (Colombia) to 4,036 (Nigeria). The latter was an especially impressive accomplishment from a small budget and reflected being able to use project workers for household interviews at no cost. Unfortunately, in research based on survey data, it is rare for authors to describe sampling procedures in sufficient detail (if at all, beyond mere sample size) to permit an appraisal of the effects on the results - in terms of biases and therefore acceptability. After some urging from the editors, most Consortium authors did provide additional information but still less than desired. The household samples selected in Brazil and the Philippines qualify as probability samples but the Nigeria sample does not, 2/ for reasons indicated in Bilsborrow and DeLargy (1985, chap. 7). Deviations from scientific sampling impair the validity of inferences drawn from analyses based on the data.

But even having detailed data over time on a project population ((c) above) is not quite sufficient. Changes in such factors as income levels, female employment, or the pace of childbearing in the project area after the project is instituted may be no greater than those in other areas of the region or country. Thus, it is preferable to determine if the change $[X_t(P) - X_0(P)]$ is greater (in percentage terms) than $[X_t(C)-X_0(C)]$. For this, one needs high quality retrospective survey data from both project and non-project areas. If project evaluation is planned a priori, it would be even better to have a panel survey - two separate and comparable household surveys in both project and control areas, the first round being the baseline survey. Because of its potential for reducing costs, it is appropriate to describe the unusual quasi-anthropological approach followed in the Thai study. Anthropology students at Mahidol University were given one month's special training and then sent to the five selected villages (one to each village) to live for three months. They observed the behaviour of villagers, conducted a census, and interviewed in detail a cross-section of (judgementally selected) young, middle-aged, and older men and women (four from each of the six age-sex groups) about their lives in the past (i.e., before irrigation) and at the time of interview. Since the use of only a few study areas can always be questioned regarding its representativity, project designers increased the number of communities and reduced the time in each in comparison with the common anthropological approach of living for a year or more in a community. 3/ This was an intriguing way of combining elements from the intensive anthropological method with those of the extensive sociological approach. Nevertheless, it is too easy to selectively collect

data to verify a priori expectations when the data are not collected on a larger, randomly selected set of households in a larger number of scientifically selected communities.

The Thai study looks at changes both across (irrigated and non-irrigated) communities and within them over time. It was unique among the five in this sense, as well as in being a community-level analysis, not an analysis across households. 4/

Questionnaire design. The issue of questionnaire design and content in the five studies could be treated in great detail (see descriptions and assessment in Bilsborrow and DeLargy (1985) but it is essential to simply touch on several highlights here. First, in the Thai project, no explicit questionnaire was used to collect community information (except the short census). Student observers were only asked to record their impressions. As shown in the discussion of the Ecuador projects below, the author feels strongly that a structured community questionnaire should be used to collect more complete and comparable information across villages. Another example is that used in the sample barangays (communities) in the Philippines, described by Herrin in Bilsborrow and DeLargy (1985).

Turning to the individual questionnaires, the Nigerian instrument has serious weaknesses in the schedules for both the man and woman, though the topic coverage is good. Fertility and infant mortality information is incomplete: there is no information on the woman's total live births, on children surviving or on dates of births. The Nigerian questionnaires, however, did carefully deal with the considerable cultural obstacles to obtaining data on family planning and desired family size. The Colombia questionnaire is impressive in topic coverage but physically cumbersome, which must have complicated interviewing and data processing. The author gathered detailed migration (including return migration) and economic information - viz., production by crop, animal husbandry, time worked and wages, costs of production, and ownership of consumer durables particularly those requiring electricity. But little was used in the analysis. The other two household questionnaires (in Brazil and the Philippines) were both detailed and well designed. Regarding fertility and mortality data, both obtained careful pregnancy histories, but neither obtained sufficient non-demographic retrospective information for analyses of type (c) above.

Questionnaire length is tied to the duration of interview. While long interviews can lead to interviewer and interviewee fatigue (or reduced co-operation) and lower quality data, usually this has not been a major problem in developing countries, particularly in rural areas (e.g., Bilsborrow and others (1984, chap. 4) and references therein). While exact data were not provided by the investigators, orders of magnitude for the total duration of interview(s) per household are probably one half hour in Nigeria; one to two hours in Colombia and Brazil; and two to three hours in the Philippines. The time in Brazil refers to the combined duration of interviews with the husband and wife, and that in the Philippines to the sum of the durations of interviews in several separate visits to the household. Such separate visits made it possible to collect more detailed information without risking data quality. Repeated visits in the Philippines were

economically justifiable because of the use of local (resident) interviewers - which is not feasible in most situations.

In concluding this section, specific information should be collected relating to the specific development project (or package of projects): for example, details on land tenure and use for appraising the effects of a colonization project, and household usage of electricity and date and cost of installation for appraising an electrification project. The information desired also depends on the demographic impact in which one is interested - e.g., migration, fertility or mortality/health - though we see no general reason for restricting the topic coverage to one or the other, since the marginal cost of a few more questions is trivial once the rest of the survey is in place.

Field-work. In table 1 above, we see that in three of the five projects, interviewers were residents or inhabitants of the communities, the two exceptions being Thailand and Brazil where people with the necessary skills were not available in the project sites. While the use of resident interviewers often contributes to higher rapport, the use of authority figures, such as military personnel (as in Colombia) or plantation foremen (in Nigeria), could yield distorted responses.

Training was apparently a common problem. For two countries, no information was available. In Brazil two weeks of training was found minimal, so the three days in the Philippines for such a complex and lengthy household questionnaire(s) appears far too little. Substantial experience now exists in training interviewers through survey programmes such as those of the World Fertility Survey (WFS), the International Labour Organisation (ILO), POPLAB, and government statistical offices around the world. A rule of thumb is that it takes two weeks' training for a short questionnaire (e.g., Nigeria) and three to four weeks for a longer one.

All projects used pre-tests meaningfully, which led to improvements in questionnaires. This was particularly crucial in Nigeria where many of the original questions made no sense to the respondents and needed to be altered. But with the complex questionnaires used in Brazil and Colombia, whether one pre-test was enough can be questioned. Pre-tests of community questionnaires are also important but are not reported as having been carried out in either the Philippines or in Thailand. As a positive example of the importance and use of such tests, Chayovan and Knodel (1985) describe a process of carefully pre-testing three times a community schedule in Thailand. There is an almost universal tendency to not pre-test adequately and to rush into the definitive field-work as quickly as possible in order to adhere to *a priori* schedules. 5/

Analysis of data

Data coding and data processing. In several cases more extensive use of pre-tests could have led to more complete pre-coding of questions (reducing the "other" that needs to be expensively coded *ex post*). But the authors provide no discussion of problems here, though coding some of the economic questions must have been cumbersome. Data processing, on the other hand, was

cited as a persistent problem - even in those developing country contexts where one would expect it to be relatively straightforward, e.g., the premier research university in the Philippines, a wealthy Jesuit university in Colombia (with the largest university population research programme in the country), and one of the two major universities in Thailand. Developments in software packages and training of programmers in developing countries are surely reducing these problems, which were paramount in the WFS programme as well. When establishing the research schedule, one useful rule of thumb could be to make careful estimates of the time for each aspect of data processing, from data entry and validation, machine range checks, sequencing checks, internal consistency checks, and subsequent data quality and consistency checks based on initial tabulations - and then multiply by two! Unfortunately, delays in data processing contributed to budget shortfalls as well as impaired data analyses.

Tabulations and statistical analysis. Table 2 illustrates some project differences in terms of demographic topics analysed and methods of analysis to date. As noted above, the utility of comparing project impact and control areas declines as one proceeds from the Philippines to Nigeria to Thailand or Colombia to Brazil.

What macro-tabulations should be made to compare areas? Answer: those that measure the demographic (impact) variables of interest as well as important intervening variables, such as age at marriage, use of family planning, female education and work, and mean production and income. Specialized tabulations are also needed on variables directly related to the particular development project - e.g., in the case of electrification, on the ownership, purchase, and use of electricity-using household or business durable goods. For example, ter Wengel documents the far faster rate of purchase and accumulation of durable goods in El Pemon than in Villa Gomez after the former was electrified in 1976. Similar tabulations could have been presented for the Philippines and perhaps Thailand. While macro-tabulations are an important first step in the analytical process, and all analyses would have benefited from more of them, the value of the information would have been far greater if pre-project retrospective data had also been obtained.

The last line of table 2 refers to whether comparisons were made before or after the project for the same population group. This was done in a non-quantitative way in Thailand and in a limited fashion for Colombia. While such basic tabular analysis is useful for forming general impressions, it is not sufficient for investigating relationships where more than one explanatory factor (development project) may influence the criterion variable.
For this, multivariate techniques are needed. But only two of the five studies have used them (e.g. multiple regression analysis) - ter Wengel, based on aggregate time-series data for the two towns in Colombia, and Herrin in the Philippines. The Colombia regression was of the form $M_i = b_0 + b_1$ Village $+ b_2$ (Friends/relatives), for i = 1970 . . . 1979/80, where M_i indicates net out-migration and Village is a dummy variable. It is a novel approach, testing whether the rate of out-migration over time changed after the project was instituted.

Table 2. Methodological comparison of projects: tabulations and analyses performed.

Topics analised in report:	Philippines	Colombia	Thailand	Nigeria	Brazil
Migration	No	Yes	Yes	Yes	Yes
Fertility	Yes	No	Yes	Yes	Yes
Family planning	Yes	No	Yes	Yes	No
Desired family size	Yes	No	Yes	Yes	No
Mortality	No	No	Yes	No	Yes
Morbidity	No	No	No	No	Yes
Macro-analytic comparisons of project and control areas:	Yes	Yes a/	Yes	Yes	Yes a/
Using tabular analysis	Yes	Yes	Yes	Yes	Yes
Using regression analysis	No	Yes	No	No	Yes
Micro-analytic comparisons of persons/households in control and non-control areas:	Yes	No	No	No	No
Using tabular analysis	No	--	--	--	--
Using regression analysis	Yes	--	--	--	--
Before/after analysis	No	Yes, indirect	Yes	No	No

a/ Indirect control group: "other city" in Colombia; other project population in Brazil.

Note: A dash (--) indicates that the item is not relevant or not possible.

Herrin extends the estimation model an important step beyond the usual household model, which is of the form $F_{ij} = a_o + > a_iX_{ij} + e_{ij}$, where F_{ij} is the fertility of the ith woman in the jth community, X_{ij} are independent variables measured at the individual or household level, a's are regression coefficients, and e_{ij} error terms. Herrin uses a "multi-level" model of the type $F_{ij} = a_o + > a_iX_{ij} + b_jX_j$, where X_j refers to the level of the community variable in j and the b_j's are the community variable regression coefficients. The Herrin approach would have benefitted from including in the regression additional community variables, such as availability/location of schools and health clinics, and perhaps also testing interactions between individual/household and community variables. In any case, the area of multi-level analysis promises to experience revolutionary developments in the near future. 6/

Finally, it is worth noting that none of the five case studies documented the economic effects of the project, though most collected the necessary data and used the A--->B--->C analytical scheme above. To the extent there are important on-going changes in the "intermediate" economic variables B, there may be related (additional) changes in demographic behaviour C that can be expected to occur in the future.

Inferring policy implications from the results

Each of the authors drew policy implications from the findings, but further analyses are desirable in all five. In some cases, the implications were clear and logically followed the author's view of his/her results--e.g., Colombia and Thailand. But in both these cases (and Brazil), the sample was rather limited for drawing strong conclusions. In the other two cases, Nigeria and the Philippines, the wealth of data is such as to encourage more explicit policy conclusions, but analysis using only two-way tables (as in Nigeria) is not persuasive. Even in the Philippines, where the multivariate results suggest a significant effect of electrification in the community on (reducing) household fertility, more extensive testing of community variables is desirable.

How does this relate to policy? Very intimately. Relationships between individual-level dependent and independent variables provide at best indirect evidence of policy effects. We must focus attention on community-level independent variables, which are close to actual policy variables: a person's education is not a policy variable, but the existence and location of a school (which indicates the convenience of obtaining an education) is. This is true of other facilities as well. In terms of large government development projects (such as a dam or electrification), the relevant policy variable is whether the village has it or not. It thus seems crucial to collect areal- or community-level data if one is to proceed very far towards policy implications. And since a development project often comes with additional infrastructure, information on changes in the other types of infrastructure (including dates of installation and usage levels over time) is needed as well. To the extent that different combinations of projects and infrastructure are expanded at different times and rates across communities, efforts can be made to estimate the separate effects if data are collected for enough communities.

The time frame is also important to bear in mind in impact analysis. The effects of colonization/settlement programmes on altering the environment and attracting migrants can be quite quick. Indeed, any development project which alters income-earning prospects can have significant effects on migration and therefore population distribution, even in the short run, because potential migrants respond to economic opportunities quickly once they are aware of them. On the other hand, in the cases of fertility and mortality, improvements in economic opportunities often require considerable time to have effects, perhaps a decade or more rather than the few years observed in most of the Consortium projects. Such lags should be carefully considered in designing surveys to collect information to assess the demographic impacts of development projects. If one wished to investigate fertility effects, for example, it would be desirable to have a significant number of years - e.g., 10 - between a baseline survey and a later survey. In the case of a single-round retrospective survey, at least a partial life history going back to the time before the project was implemented is necessary (as in the Brazil project).

It is clear from the five case studies that there are potentially significant lessons for policy makers. Identifying the separate effects of development projects from past project analyses can help in estimating the expected population impact of similar projects still in the planning stages. If the impact is expected to be unfavourable, changes in project implementation may be able to ameliorate the negative consequences or maximize the positive ones. 7/ In countries that have specific population policies and goals, impact analysis can help identify economic development projects which undermine or are in conflict with population goals and thus influence the choice of projects at the margin. For example, some development projects might be equivalent in terms of meeting economic goals, but one may also lead to lower fertility or mortality or a better population distribution and thus be preferred.

Conclusions from the International Consortium

During the evolution of the five field studies and the book (Bilsborrow and DeLargy, 1985), all the authors came to recognize that population impact analysis is very complex. Therefore, while very small-scale and inexpensive pilot studies (such as the five here discussed, whose budgets were generally in the $10,000 to $30,000 range) can collect useful data and lead to provocative hypotheses, they can rarely yield sufficiently clear findings for policy making. It was therefore urged that greater resources be allocated to do the job better - that is, at the outset of a development project sufficient funds should be allocated (e.g., 1 per cent of the project cost) to collect baseline data on a probability sample, ideally comprising significant numbers of both households and communities in both project-impact and control areas, to be followed some 5 to 10 years later by a separate survey (perhaps a partial panel). The alternative is a large-scale, single round, post facto survey with intensive, retrospective data collection in a number of both project and control areas to inquire about conditions prior to the project as well as at the time of interview. This has the advantage of not requiring a

long funding commitment over time but may (a) encounter memory recall problems of respondents; and (b) suffer distortions because of changes that have occurred in the population composition of the project impact area or in socio-economic conditions resulting from factors other than the project.

The five case studies and other survey experience of the author suggest many other, more specific, characteristics of a desirable survey for collecting data for meaningful population impact analyses. These include a detailed multi-purpose household survey instrument, 8/ collecting information at the community as well as the household level (see below), using probability sampling methods to select households, careful training of interviewers, planning enough time for collection of data in the field (to avoid rushing and its effects on data quality), allocating sufficient project time and funds for both data processing and analysis, 9/ using both simple tabular and more complex multivariate techniques to attempt to separate out project effects from the effects of other concomitant changes, and presentation of the findings to government officials in a clear, concise and policy-relevant manner.

Evaluating the demographic impact of development projects: using areal and community-level data in Ecuador

Ecuador has an ambitious programme of integrated rural development projects, begun under the Roldos-Hurtado Government, with explicit goals to reduce rural-urban out-migration from project areas and to improve living and health standards. There are 17 rural development projects in all areas of the country - the Sierra or Highlands region, the Costa or Pacific coastal plains, and the Oriente or Amazon headwaters region in the east - agreed on by the Government of Ecuador and several international agencies, mainly the World Bank, the United States Agency for International Development, and the Inter-American Development Bank. Approximately 40 per cent of the funding is provided by the Government of Ecuador and 60 per cent by international agencies for each project. The initial agreements began to be signed in 1978-1979, and the first actual project activities in the field began in 1981. Several of the 17 projects commenced only in 1985-1986.

The 17 projects cover geographic areas ranging from about 20,000 hectares (ha) to 650,000 ha. All are integrated rural development projects (proyectos de Desarrollo Rural Integral (DRI)); 16 are administered by the Ministerio de Bienestar Social and one, the largest, by a provincial government (Consejo Provincial de Pichincha), both with headquarters at Quito. The National Planning Agency (Consejo Nacional de Desarrollo (CONADE)), through a population unit directly responsible to the Minister and funded partly by UNFPA, is evaluating the demographic effects of the DRI projects. 10/

Given limitations of funds and time, 3 of the 17 DRI projects could be selected for evaluation. This was done in consultation with project authorities, utilizing extensive project documentation and taking into

account the two-fold requirements of selecting projects in diverse agro-climatic zones but with as long a history as possible (so that there would be effects to appraise). However, it is clear a priori from the dates of completion of project components that in 1987 there will be time to appraise only short-run effects (over three to five years). While significant effects on out-migration and possibly health/mortality may be expected in such a period, effects on fertility are unlikely. Moreover, the projects do not have explicit fertility goals, nor do they include family planning components, except passively, in that methods are supposed to be available at new health clinics established by DRI.

A second problem in evaluating the demographic effects of the DRI projects arises precisely out of their integrated nature. That is, project components may include any or all of the following: construction of irrigation canals and ditches, drinking water purification and storage facilities, health clinics and rural health posts, construction or repair of schools and classrooms, creation of athletic facilities and community centres ("casas comunales"), feeder road building and repair; technical assistance from agronomists, including creation of demonstration projects for new crops (hybrid or improved seeds), fruit trees, and animals (commonly, literally, guinea pigs!); animal nutrition advice and vaccinations; assistance in commercialization of agricultural products, obtaining credit and legalization of land titles; and assistance from social workers in creation and expansion of campesino (peasant) organizations. The plethora of such activities makes it very difficult to relate any specific demographic effect, even if observed, to a particular project component. Nevertheless, it should be noted that despite such a comprehensive list, some common components of rural development projects are rarely included in the DRI projects in Ecuador, viz., electrification and construction of schools and feeder roads. The most common components in DRI are irrigation, water supply, and technical assistance in agricultural production and social organization.

Partly because sufficient resources were not available to implement household surveys and partly out of personal interest, we selected community-level surveys as the principal methodology for attempting to assess the demographic effects of the DRI projects. But in the course of initiating the project, other sources of potentially useful data came to our attention. Since these are also likely to be available in varying degrees in other countries which have rural development projects, we first briefly describe how these data may be useful in Ecuador and their limitations. These data include government administrative statistics, followed by project data and specialized community surveys.

Government administrative statistics

Statistics are collected by government agencies in the normal course of their activities, usually every year, though for ease of exposition census statistics are included here. Statistics of possible relevance for the present purposes include the following, with the responsible government agency in Ecuador indicated in parentheses: health facilities and health

service utilization (Ministerio de Salud Pública (MSP)); family planning acceptor statistics (mainly MSP and two private organizations); education statistics-facilities, teachers, and enrollment by age, sex, level, grade, location etc. (Ministerio de Educación y Cultura); vital statistics and population (Instituto Nacional de Estadísticas y Censos (INEC)); agricultural production and land use (Ministerio de Agricultura y Ganadería and INEC); and employment (Ministerio de Trabajo). While the value of all such statistics depends crucially on their quality and completeness (as well as their continuity and the shortness of lag between the year of reference and the year the publication becomes available for evaluating current effects of an on-going project), even incomplete statistics can be useful indicators if their completeness has not changed during the time period of interest.

The statistics with the highest potential value for demographic evaluation are the vital statistics. In Ecuador both birth and death registration have been 80 to 90 per cent complete since early in the twentieth century, without apparent trend. (Unpublished 1986 data are now available from INEC.) This suggests that registration data may be useful for approximately measuring numbers of births and deaths in pre- and post-project years to determine the extent to which fertility and mortality (especially infant) have changed. To compute rates, denominators have to be estimated independently from other project data (see below) or extrapolated from previous census data. What we do not yet know in Ecuador is to what extent the registration data are complete for the particular parroquias where the DRI projects are located, and whether this completeness has changed over the 1981-1985 period: INEC demographers believe it has not changed for the country as a whole, and this was also stated by the one registrar interviewed in a project area in August 1986 in a pre-test, but it is still hoped to check the actual registration certificates.

Of course, registration statistics in most developing countries will not be adequate for this purpose. Moreover, their completeness may improve over time because of linkages with the development projects themselves, but the two operations are totally independent in Ecuador. Since vital registration data can potentially provide information on two of the three key dependent variables of interest and since data on fertility and mortality from other sources may be particularly unreliable, it is important to examine the usability of vital registration statistics carefully.

On the other hand, it is not likely that useful health and family planning statistics will be available from standard sources in Ecuador. They have yet to be brought together from the various providers in a consistent time series, much less presented in highly geographically disaggregated form. Also problems in measurement and interpretation are pervasive with health data: does an increase in attendance at a health clinic mean there is more sickness or only more awareness of the importance of better health?

Useful education statistics are available in Ecuador, even at the subdistrict or parroquia level (though unpublished). 11/ Most agricultural and (rural) employment statistics are, however, generally available only for years of agricultural censuses (the only one being in 1974) or population

censuses. The fact that the last population census was in November 1982, provides a good amount of approximately baseline statistics on many items, but there is nothing since to compare them with. One remaining possible source is data from a continuous, on-going (annual) project of INEC on agricultural production (SEAN), if any of its (cluster) sample points are located in the parroquias where the projects are located.

In the evaluation of the DRI projects in Ecuador census data will be of limited value because of the short and recent duration of the projects. This precludes the major potential value of census data, which requires using data from two comparable censuses to assess changes in project (and non-project control) area populations over time. Since censuses in most countries are 10 years apart, census data are useful only if the projects have been in existence more than 10 years, with the initial census ideally corresponding to the beginning of the project, thus providing baseline data. Census data will also be more useful to the extent that they can be tabulated for micro-areas, or areas that correspond to the projects.

Project data

For the two selected DRI projects under its jurisdiction - Salcedo and Quimiag-Penipe - the Ministry provided a detailed Diagnóstico (baseline assessment of the project area, around 1981); annual reports listing all infrastructure construction and technical assistance activities; and a mid-period evaluation (1983-1984). Unfortunately, no "final" project evaluation is available as yet. Since major international funding for these two projects ended in 1986, some form of final general evaluation is also likely. Information in both the Diagnóstico and the mid-project evaluation was based on project data, small sample surveys of households receiving various types of project services, and direct observation (and the 1974 Agricultural Census for the Diagnóstico). Interviews were held with household heads, the total number of interviews amounting to 428 in Quimiag-Penipe. Information is available on the initial population size, housing, characteristics, education facilities, illnesses, peasant organizations, other infrastructure, main products, and average net incomes per UPA or farm. Activities in the early part of the project (infrastructure and services provided) are tabulated, including percentage of population covered, for each community in the project area. For example, in Quimiag-Penipe there are 33 communities, most receiving rather limited project services. Despite the considerable volume of information, it is of limited value for the present purposes because (a) it does not cover the full period up to 1986, and (b) no information whatsoever is obtained on any of the three demographic variables, because project goals were exclusively economic and social.

In the case of the third DRI project, the largest covering the canton or district Santo Domingo de los Colorados, a similar Diagnóstico was prepared with dozens of maps showing pre-project (1980) infrastructure, population, altitude etc. Because this project has more resources, it has a full complement of administrative and subject matter specialists in the Consejo headquarters in Quito as well as in the field. Besides the usual complement of agronomists, health workers etc., the field office also employs many

social workers, each responsible for three communities. They spend four of the five days each week in the communities. They have had time to collect and write down some information about their communities, in September 1985 and April 1986, for different communities using two different forms. Information collected includes physical aspects, population (and mean family size and density), quality of housing, whether a health facility is available, schools (students, teachers and classrooms), bus service, churches, electricity and drinking water. Short text descriptions are available on the recent history of the community and economic and social activities. The 1986 form adds the origins of in-migrants and names of community leaders, and tells where people may go if there is no local health facility; but it carries less information on housing and economic characteristics than the 1985 form.

Some of this information could be compared with that in the Diagnóstico, which shows, for example, the total population and whether the community has electricity, a health clinic etc. But, unfortunately, the information does not appear to refer to all the population of the community area, only that living in the town centre. Thus, information on some (though a minority of) households is excluded. Also, no information is given on land use, actual production levels, employment, the three demographic rates or age distribution.

There is an important lesson: researchers should plan what they are likely to want to assess at the outset and then collect the necessary information in the baseline to be able to compare it with information collected later. This should include, for any evaluation and not just one focusing on demographic effects, the population by broad age-group and sex, and health, mortality, fertility and migration levels, because these are key measures of the welfare of the population in project areas (e.g., Chikira (1980) and various World Bank, US AID etc. reports).

Specialized community-level data collection

The types of information to be sought are based on a detailed extension and disaggregation of the simple conceptual scheme described above. Specifically, after reviewing the DRI annual reports to determine its range of activities, three detailed flow charts were prepared of the pathways by which various construction projects and technical assistance activities might influence internal migration, mortality and fertility. It is crucial to undertake such an exercise to ensure that all the desirable information will be sought in the community-level questionnaires. The experience of the five case studies (see Bilsborrow and DeLargy, 1985).

Next, a list of topics was prepared on which it was decided to seek information in the field. Visits were paid to the project site headquarters. This was done after it has been determined which three project areas were to be evaluated, on the basis of discussions with top DRI officials in Quito, and after review of the Diagnósticos and annual reports, and determination of the age of each of the 17 projects and dates of implementation of project components and their nature. The latter was not irrelevant- one of the older

projects under consideration (in Tungurahua Province in the central Sierra) was rejected because it consisted almost entirely of irrigation construction and had no health or education components whatsoever. For the present purposes projects were wanted which included some of the latter types of social infrastructure as well as purely economic components.

An important aspect of community data collection, especially where resources are so limited, is the selection of communities. 12/ It was determined that, to evaluate the population impact in three project areas, the set of community-level questionnaires could be implemented in only 9 communities per DRI project site, or a total of 27. Since the number of communities in each area varies from 32 to over 60, a sampling procedure is needed. For a small sample, a judicious judgement sample is superior to a random sample, to ensure selection of different kinds of (more heterogeneous) communities (Kish, 1965). The communities were first stratified according to the level of DRI project activities they received 13/ - well served, medium served, and little or not at all served - then two to four from each stratum were selected in each of the project areas. The original plan has to include a sample of nearby "control communities", but it was believed that adequate natural controls could be achieved by selecting some communities in the project area with no project activities, taking into account transportation linkages to served communities.

It is important to note that the three projects were selected on the basis of age and diversity of agro-climactic conditions, and not on the basis of ease of access from Quito or closeness to each other. The three selected were: Quimiag-Penipe, comprising two parroquias with those names near Riobamba, Province of Chimborazo, in the central Sierra; (canton) Salcedo in the central Sierra Province of Cotopaxi, closer to Quito (two hours by car); and canton Santo Domingo de los Colorados, in the coastal lowlands in the Province of Pichincha, west of Quito.

The questionnaires were developed based on field visits, and those that were in the first pre-tests were on the following subjects: institutional aspects; land tenure and production; employment, migration, and income; (other) demographic aspects; education; and health. As a paper will be prepared on the experience of designing, pre-testing several times and revising these questionnaires (Bilsborrow and Ruiz, 1987), and as the process is still on-going as of this workshop date, only a few comments are offered here regarding problems encountered and efforts at solutions.

First, it was found desirable to complete as much information as possible from project files and interviews with the project officials before going to the communities in the field. On the basis of the pre-tests, it was determined that the appropriate community respondents in rural Ecuador were as follows: first, the teniente político (appointed political leader) in larger towns, and the director of the junta campesina, or peasant board of directors, in smaller towns, can provide most information, including project activities in the community, estimates of population and migration tendencies, land distribution, main crops grown and changes since the project began, land distribution, and credit, wage and income levels and changes etc.

Since there may well be biases or uncertainty, it was found preferable to interview several campesino leaders in the junta together (see also Chayovan and Knodel on Thailand and earlier experiences in India and Ecuador, cited in Bilsborrow and others (1984, chap. 11)). Secondly, virtually every community (localidad) in Ecuador, no matter how small, has a primary school (even if incomplete) and a teacher. Teachers are often knowledgeable about many aspects of the community besides education, so they could be asked about the above topics on which they consider themselves knowledgeable. Finally, regarding health, including levels and trends in mortality, fertility and use of family planning, the only adequate respondent is usually a doctor, nurse or other health worker. If there is no health facility in the community, then the previous respondents must be asked a reduced set of health questions. 14/

The questionnaires were formally pre-tested and based on that experience were to be further pre-tested. The questionnaires were revised and simplified (the preparers had been too ambitious, seeking both demographic and economic data in too much detail). After revision, the questionnaires were again pre-tested before full-scale implementation.

Regarding the various types of data collectible in small communities, pre-tests suggested that, unfortunately demographic information would be difficult to obtain from respondents. Thus, attitudinal information may need to be resorted to on such matters as levels and trends in birth and death rates, use of family planning and desired family sizes, and major illnesses. In general, data on education and health aspects - infrastructure and services available, utilization/coverage - appear reliable. Economic data - necessary for B in the A--->B--->C scheme shown earlier - as usual present certain problems. Land distribution was not available with the disaggregation sought a priori, and, moreover, the detail appropriate in one area differs from that in another: Quimiag-Penipe and Salcedo are characterized by minifundia (most holdings are under 5 ha. or even 2 ha.) while few farms in Santo Domingo, an area of recent, rapid colonization, have less than 20 ha. We have given up on seeking agricultural output or income levels by land size strata, but key respondents do appear to know well the levels and changes in production and productivity by crop, technology and wage levels. Therefore, changes in income can be approximated. Some information about migration appears obtainable, but numbers (or percentages) appear difficult to get reliably. In a pre-test in Quimiag, we encountered conflicting opinions about whether young persons were leaving the (larger town) area more or less since the project was implemented.

It is clearly desirable to implement household surveys in a sample of households in selected communities, and compare the information with that obtained through the less-expensive community survey approach, to test the reliability of information collected in the latter. Certain topics, in particular, one would expect a priori to be more reliably obtained at the household level, such as levels and trends in fertility and use of family planning; land ownership and distribution; out-migration and reasons for it; attitudinal variables regarding care received at health centres; value of technical assistance received from DRI agronomists; quality of leadership in

the community, aspirations for children's education etc. Efforts are now under way to achieve such a household survey, to achieve this crucially needed methodological (even if very limited) comparison. This will greatly enhance knowledge of the reliability of the community data and help to identify where problems exist and where further, or more circumspect, work is necessary.

Conclusions (or rather beginnings)

The present author is less sure about the two topics of this paper at the end of it than when it was started - whether the topic is how to (in real-world situations) reliably estimate the demographic effects of rural development projects, or how to collect useful community-level data (for that or some other purpose). The first part of the paper summarized what was accomplished in five small case-study projects carried out by well qualified investigators in developing countries, mostly based on small household surveys. The second part dealt with a project, of similar small size and budgetary magnitude, which attempted to utilize existing data and specialized community-level surveys for the same purpose. Eventually, in most circumstances, it will be possible to collect and analyse community data at a much lower cost to achieve at least the same level of reliability of evaluation of the demographic effects of development projects. But at present, experience in community-level data collection is in its infancy, and cultural conditions and institutional factors that influence survey design vary even more from one country and cultural community context to another than they do across households. A household is a household (whether extended stem or nuclear, patrilineal or matrilineal etc.), but a community in country X is very different from a community in country Y in definition and size, reliability of boundaries, political organization and leadership, data availability, identification of appropriate respondents, linkages to higher-level political units and interest and ability to implement national policies.

Notes

1/ This is not to ignore many other valuable experiences of the Carolina Population Center (CPC) in household survey data collection and analysis, including the POPLAB programme, the intensive Bicol survey (see Popkin and others, 1979), the three-country CPC migration projects etc.

2/ The latter is hardly atypical. Only a small fraction of the migration survey data sets reviewed for the ILO book are probability samples (Bilsborrow, Oberai and Standing, 1984, chap. 4).

3/ The authors intended to collect data from about 30 project communities and 20 control sites but did not receive the necessary funding.

4/ Herrin's project collected quite rich community data which could be used for comparative community-level (cross-community) analysis, but this has not been done to date.

5/ Associated with each pre-test should be a written report describing procedures, providing simple tabulations and indicating problems encountered and changes adopted. This would assist in providing information useful for future questionnaire design. See also the discussion of data collection in the latter part of this paper.

6/ See papers in Casterline (1985), Bilsborrow and Guilkey (1987), and works of W. Mason, A. Hermalin, and B. Entwisle cited therein.

7/ An interesting example comes to mind. In the case of the rural electrification project in the Philippines (and in contrast to Colombia), the Government decided that the beneficiaries should pay fully for the installation costs of electricity and provided funds which families could borrow. During the subsequent 6-15-month repayment period, families became especially aware of the opportunity costs of having another child or of permitting their young children to marry. This seemed to hasten the adoption of family planning which, once begun, tended to be continued. Requiring families to pay for the projects they benefit from, and providing the funds for them to borrow, may thus be a useful way to maximize the demographic impact, as well as economize on scarce government funds.

8/ Multi-purpose survey data could be used to better appraise the economic effects of the development project as well. Besides the instruments used by International Consortium investigators (especially that of Herrin), one can cite many other possible examples of prototype multi-purpose questionnaires to draw on, including Anker (1980); Bilsborrow and others (1982) on fertility; Bilsborrow, Oberai and Standing (1984) on migration; and the World Bank's LSMS questionnaires, such as were recently implemented in Côte d'Ivoire and Peru.

9/ Bilsborrow (1974) and others have noted the omnipresent concentration of resources on data collection rather than analysis years ago. The problem has, if anything, become worse.

10/ The Rural Development Division of CONADE is also responsible for evaluating the DRI projects in more general terms, including administrative and budgeting aspects. Since such a topic is within the scope of population policy analysis activities of the RAPID project, a contract was signed by CONADE and RAPID II in July 1986. RAPID is an acronym for Resources for the Awareness of the Population Impact on Development, a five-year project (1983-1988) supported by the Office of Population of USAID. The prime contractor is the Futures Group, to which CPC is a subcontractor, responsible for activities in Ecuador and four other countries. The author is associated with the latter organization.

11/ Ecuador has 20 provinces, or states, each comprising various districts or *cantons* (120 in the country), which are in turn divided administratively into *parroquias* or subdistricts.

12/ Similarly, we would like to be able to spend more than a few days in the field per community. But we cannot afford to spend, for example, a month per community as in a more intensive anthropological approach. Interesting quasi-anthropological approaches to community-level data collection have recently been used in Colombia and Peru by the Comitato Internazionale per lo Sviluppo dei Popoli in Rome. See CISP-CVC (1976) and CISP-ELECTROPERU (1926). See also Chikira (1980), Lewis (1985) and Caldwell in Casterline (1985).

13/ This was based on project annual reports and subjective rankings of project leaders in the field.

14/ In Ecuador there are still no regular visits to households from health or family planning outreach workers.

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Annex

INTERNATIONAL CONSORTIUM OF RESEARCH CENTERS
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*The studies referred to in the text are contained in Bilsborrow and DeLargy (1985).

C. Assessing the demographic impact of development projects: the IDRC experience

Carol Vlassoff*

Over the past eight years, Canada's International Development Research Centre (IDRC) has funded about 20 research projects on the impact of development policies and projects upon household demographic behaviour, focusing particularly on poor rural areas of third world countries. About three quarters of these projects have now been completed and reports have been written on their results. The remainder are still in progress.

Most of the completed studies were conducted within a loose network of research projects involving researchers from Asia, Latin America and Africa. The present Workshop is, in fact, timely, because it coincides with the completion and submission of a manuscript incorporating a selection of these studies to IDRC for publication consideration. As co-editor of the manuscript, the author has come to know these projects well and has reached some conclusions regarding the value of such research in terms of its methodological approaches and substantive findings. In the present paper three studies have been selected: two (Philippines and Bangladesh) from the above-mentioned network and one (Thailand) which was funded independently of the network. Although these projects shared fairly similar objectives, they adopted different methodological approaches. Taking the three as case studies, it will be argued that certain methods have been shown to be superior in that they cast greater light on the complex interactions between development projects and demographic behaviour, and that they are therefore of more direct utility to policy makers.

In the following section the three studies are briefly described: their objectives, research design and main findings. The third section of the paper describes the kinds of results obtained from the studies in terms of their theoretical and practical insights, and relates these observations to the methods employed. In arguing for the superiority of certain approaches, it should be emphasized that no criticisms are intended of the particular studies in terms of their inherent value or the competence with which they were conducted. In fact, one of the reasons for selecting these projects is their essentially high quality and scientific rigour. The point is rather to argue that even though all approaches have certain merits, some have comparative advantages for this kind of impact analysis. The paper concludes with a few general observations regarding what has been learned so far about the impact of development programmes upon demographic behaviour from the IDRC experience more broadly.

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Three Asian case studies

Impact of reforestation on child mortality and fertility: Thailand

Thailand's reforestation programme is one of the most important development projects in the country. In order to make way for agricultural expansion, rapid deforestation took place over the past two decades, creating a precarious environmental situation in terms of ecological balance, climatic conditions and water resources. In order to address the problem the Government of Thailand introduced a reforestation programme during its first national economic and social development plan of 1961-1966, and this programme has continued to the present time. The programme sets targets to be met by each five-year plan, aimed at reforesting a designated number of rai (2.4 rai = 1 acre) through the establishment of national forest parks and "reforestation villages".

Until 1967 all reforestation activities were conducted by the Royal Forest Department but thereafter two state enterprises, the Forest Industry Organization (FIO) and the Thai Plywood Company, were permitted to undertake reforestation projects. The FIO, a governmental enterprise financed by profits from timber sales, took primary responsibility for the programme. The FIO recognized the dual, conflicting demands being placed on forest lands by the rural inhabitants for agricultural expansion and for reforestation to maintain the forest stock. Thus, in addition to developing teak and other wood plantations, the FIO began in 1968 to establish "forest villages" for the rural inhabitants.

The FIO and Royal Forest Department reforestation programmes and associated forest villages were intended to assemble and settle rural populations and shifting cultivators in certain areas to prevent further clearing of land; to provide, through the creation of forest villages, a ready supply of labour for forest improvement activities, including nursery work, planting, weeding, pruning, thinning and pesticide spraying; to improve the economic and social conditions of the settlers through the provision of amenities such as water supply, electricity and roads, social services including health care, family planning and education, and wage employment opportunities related to forest activities; and to encourage the inhabitants to grow crops both for subsistence and for additional cash income. To this end, each family was provided with a plot of land. Crops grown would be similar to those normally grown by rural inhabitants, such as rice, ground nuts, beans, tobacco, tapioca, and other types of fruits and vegetables.

Given the nature of the forest village scheme (provision of land, employment opportunities, amenities and social services), it was hypothesized that significant economic and social benefits, such as greater household income, better health and higher educational levels, would result. It was also expected that the changes in economic and social development would have important demographic implications, especially with respect to infant mortality, a very sensitive indicator of the level of socio-economic development, and fertility.

The principal objective of the study, undertaken by Pisit Sukreeyapongse (1984) and his colleagues at the Faculty of Environment and Resources Studies and of the Environmental Social Sciences Program at Mahidol University, was to assess the social, economic and demographic effects of the reforestation programme in northern and north-eastern Thailand, the regions where problems of deforestation had been most severe, and to recommend appropriate improvements in the reforestation programme based on the research findings.

The methodology consisted of the selection of a two-stage stratified sample, which included 90 villages and 60 households per village, representing 30 reforestation villages, 30 adjacent villages affected by the reforestation programme in so far as their residents obtained employment in the reforestation activities, and 30 distant villages not involved in or influenced by the programme. A cross-sectional survey included interviews with village chiefs, community leaders, household heads and wives of household heads. Data were collected from 5,360 households, about 2.3 of which were located in the northern region, and 1.33 in the north-east. The data were analysed by means of multiple regression and path analysis.

The main findings of the survey may be summarized as follows:

(a) Rural-urban migration was slowed by the reforestation programme, illustrated by lower out-migration from the reforestation households than from the non-programme ones;

(b) Incomes and employment were higher in the reforestation villages than in the other communities;

(c) The reforestation programme benefited not only the reforestation villages but also the surrounding communities by providing employment, additional income, and better communications;

(d) Social problems, including drug addiction and thievery, were more prevalent in the reforestation villages than in the non-programme areas;

(e) Child malnutrition and morbidity were more prevalent in the reforestation villages;

(f) There was some indication that fertility in the reforestation villages was lower than that of the other communities, at least in the northern region; although among women aged 35 and over fertility was higher in the programme villages. In the north-east, however, fertility was higher in the reforestation areas than in the others, perhaps because the former were relatively less developed and poorer than the more established communities. Within the reforestation villages themselves, employment of women in the reforestation activities was negatively related to their recent fertility, implying that the reforestation programme was having a depressing impact on previously high levels of childbearing in these communities.

Impact of the Comilla Programme: Bangladesh

The Comilla Programme was established in 1959 by the Pakistan (later Bangladesh) Academy for Rural Development (BARD) as a pilot project to increase the agricultural production of rural cultivators, both to improve their incomes and to enlarge the nation's food supply. It involved a broad-based integrated rural development programme in the previously backward district of Comilla-Kotwali, and included agricultural extension, credit co-operatives, the introduction of improved varieties of paddy, and supplementary inputs such as irrigation, fertilizers and pesticides. The physical infrastructure was also strengthened to include better educational facilities, opportunities for female employment and family planning services.

The objective of the study, conducted by Barkat-E-Khuda (1986) of the University of Dhaka, was to examine the impact of the Comilla Programme on demographic behaviour in a selected village, Sreebollobpur, in the programme district. The village had been exposed to the BARD programme for about two decades, and had a population of 1,466 persons at the time of the survey in 1979. The methodology involved both a micro-level study, using participant observation, and the collection of quantitative data by means of questionnaires. A census of households was undertaken, followed by several highly focused smaller surveys covering 50 to 70 per cent of all households. The quantitative data were analysed mainly in the form of cross-tabulations.

The principal results of the study were as follows:

(a) The most dramatic changes arising from the agricultural development programmes were economic: rice production per acre increased as a result of the application of fertilizers, pesticides and irrigation; and high-yielding varieties were adopted by 75 per cent of the households. Demand for labour reduced unemployment in the area, and incomes were higher than those of the general Bangladesh population;

(b) Levels of literacy increased, as did access to and utilization of health facilities; tubewells replaced contaminated sources of drinking water and better communications allowed for the diffusion of modern ideas;

(c) Contraceptive use was much more prevalent in Sreebollobpur than in Bangladesh as a whole, the pill and condom being the most popular methods;

(d) Fertility was considerably lower in the study village than in the nation overall: Sreebollobpur women aged 40-44, for example, had borne 6.0 children on average in 1979, compared to 7.1 for Bangladesh in 1975/76;

(e) Villagers generally had lower fertility ideals and were less traditional with respect to son preference than had been found in surveys conducted elsewhere in Bangladesh.

Impact of the Green Revolution and family planning on two rural municipalities: Philippines

In a longitudinal study of two rural areas of Southern Mindanao, Magsaysay and Matanao, Robert A. Hackenberg (1986) examined the impact of a number of development activities over the 1970-1980 decade. At the time of the baseline study in 1970 the two municipalities were characterized by different levels of development. Both had experienced heavy in-migration as a result of a government policy to encourage frontier expansion and to accommodate a rapidly growing population. Between 1948 and 1960, more than two million migrants homesteaded farms in Mindanao. In the mid 1960s, owing to the depletion of land reserves, the frontier was closed to further settlement.

Magsaysay, the more progressive of the two study sites, was the first village in the area to accept miracle rice and hand tractors. Moreover, its industrious population of Ilocano migrants were familiar with irrigated rice technology. Matanao, by contrast, was a hilly upland area where Cebuano migrants relied upon the production of three corn crops per year and where the threat of soil depletion was imminent. Both the Ilocanos and the Cebuanos originated in areas of very high fertility and, as labour was in demand and the age structure of the population young, it seemed probable that rapid growth would continue for some time. However, at the time of the baseline survey, Hackenberg hypothesized that earlier fertility reductions would be evident in Magsaysay than in Matanao owing to its greater progressiveness and comparatively greater opportunities to better its economic circumstances.

The surveys included a sample of 2,050 households divided between the two communities. Interviews incorporated both economic and demographic variables, and covered all household members. The author, an anthropologist, also retained continuous contact with the region over the 10-year period, giving his analysis the richness and intensity of an in-depth micro-study. The data were analysed mainly by cross-tabulations.

Over the decade, three important government programmes were introduced which seemed to make the hypothesized fertility differences between the two communities even more likely. These included land reform for rice and corn farmers, limiting their holdings to seven acres, crop production loans to rice producers and a nation-wide family planning programme. Whereas for the rice producers, the loss of land was compensated for by the increased yields made possible by Green Revolution technology, there were no offsetting benefits for corn producers; similarly, crop production loans were not available to corn growers. Hence, it was hypothesized that these programme interventions "would serve to hasten the differentiation of Magsaysay and Matanao along the lines predicted" (Hackenberg, 1984).

The results of the research were quite surprising, especially the changes produced in cropping patterns, levels of living and fertility. These changes are summarized, albeit too briefly, as follows:

(a) The effects of land reform were more pronounced in Matanao, the "control" area, because farmers made major shifts to other cash crops, whereas Magsaysay producers retained their previous cropping patterns;

(b) The proportion of tenants and leaseholders decreased as a result of land reform in both communities, while that of labourers increased markedly. The latter organized themselves into contract labour gangs or shifted to share and wage arrangements;

(c) Income distribution improved in both communities owing to production increases from the high-yielding varieties and substantial gains in employment opportunities for both men and women;

(d) With the increasing specialization of adult manpower, child labour declines; in fact, children of farm labourers did the least productive work, and those of high income families did the most;

(e) Out-migration was experienced in both areas, mainly of 20-29 year olds in search of urban jobs, especially from the highest income households;

(f) Fertility fell dramatically in both communities, but the decline was greater in Magsaysay than in Matanao. In the former, total fertility rates for all women dropped from 7.5 to 3.3; in the latter, from 7.5 to 4.1;

(g) The use of effective contraception was higher in the study areas than elsewhere in the Philippines;

(h) Educational levels increased in both areas, especially among females, and this was accompanied by a "breathtaking shift downward" in marriage rates.

Evaluation of methodological approaches in terms of insights gained

The Thai study sought to obtain generalizable results by selecting a very large sample, representing different districts and types of reforestation and non-programme village. Hence, over 5,300 households were interviewed. As a result, some very interesting findings emerged regarding the relationships between residence and work in a reforestation village and fertility and child mortality, providing challenging hypotheses for further investigation.

On the other hand, many questions were left unanswered by the Thai project which could be investigated more appropriately by a smaller scale and more intensive study, involving longer residence in the communities, observation of daily life and closer acquaintance with the villagers. For example, the surprising finding that child morbidity and malnutrition were higher in the reforestation communities than in the other villages is not explained in any depth. We have the impression that this may be due to the relatively short length of time the villages have been in existence and therefore to the lack of a supportive community infrastructure, both physical

and social. However, it may also be that the residents of reforestation villages have different characteristics than those of the more settled communities, and perhaps adopt different behaviour with regard to, for example, feeding practices and child-care. It could also be that the higher employment levels of women in the programme villages interfere with their ability to look after their children adequately. None of these hypotheses was investigated in the Thai survey.

Another interesting finding of the study which deserves more attention is the somewhat contradictory picture of the programme's impact upon fertility. What are the reasons that, in the northern region, fertility was higher among older women in the reforestation areas, but lower among younger females? Why was overall fertility higher in the programme villages of the north-eastern region? Only a brief and hypothetical explanation was given by the research team, presumably because their data did not allow them to reach any firm conclusions about these relationships. The hypothesis was provided that the relative poverty of the reforestation villages could have been responsible for this higher fertility, but this was not developed or investigated in any detail.

Generally, also, the Thai study leaves open the question of the nature and extent of interaction between the reforestation programme and demographic behaviour: evidently, the economic effects were positive but the demographic effects were much less straightforward. In the case of child mortality and morbidity, in fact, the reforestation programme seemed to be having a clearly adverse impact.

The Bangladesh study employed both quantitative and qualitative methods to examine the impact of the Comilla Programme on a selected village. As such, it was able to illustrate that the programme was creating better living standards and educational and other opportunities than were found elsewhere in Bangladesh. Because of the author's familiarity with the village residents, case studies were provided, which allowed us to understand, to some extent at least, the reasoning of local farmers concerning, for example, child-bearing and the economic value of children. Although the present paper could not provide a detailed reiteration of the Khuda study, explanations are given of how development activities have led to the eventual lowering of fertility through the transformation of traditional ideals and the adoption of family planning, which was readily available in Sreebollobpur.

On the basis of this study one can reach tentative conclusions about the impact of the Comilla Programme. However, in order to be more confident about the reliability of such observations, a larger sample would be required, along with a more scientific research design. The latter could either take the form of a longitudinal study, in which a baseline survey is conducted at the inception of the development programme and changes are observed over time, or of an experimental design, in which a control area is selected as a point of comparison with the intervention area. Since neither of these approaches was adopted, the author was forced to compare his findings with averages for the nation as a whole and with results from other studies, all of which were conducted at times different from those of his own survey. Although such comparisons allow one to conclude that the development interventions are

having a positive impact, one cannot say exactly how much of the change is due to the programme, and how much would have occurred without it, as a result of overall modernization tendencies or other changes in the society.

The Philippine study utilized a combination of a longitudinal design and an experimental approach, in which one community was taken as a control area. It also combined quantitative surveys of a relatively large population with micro-level observation over a 10-year period. This mix of methods not only yielded reliable quantitative data on the economic and demographic changes resulting from the Green Revolution and the family planning programme in the two communities, but also provided the basis for understanding larger contextual influences and for interpreting unexpected findings. This study therefore presented a broader and more coherent portrait of regional development and its consequences than the other two projects.

Some examples may serve to illustrate this point. Hackenberg was concerned not only with economic and demographic adjustments but also with a more theoretical understanding of the meaning of development, and behavioural responses thereto, at the regional level. Thus, much of his paper dealt with the adequacy of various theoretical interpretations of these adjustments, including "polarization" and "stratification". However, his micro-level trend analysis led him to conclude that "diversification" is a more accurate description of rural dynamics in Mindanao society. Peasants reacted in a number of innovative ways to changing circumstances and opportunities. For instance, tenants willingly gave up their land in order to take up other, more profitable, contract and wage arrangements. Thus, by seeking evidence for various broad theoretical perspectives at the micro-level, Hackenberg derives a more meaningful framework for the entire analysis, that of diversification, than would have been possible through the adoption of existing interpretations of regional patterns.

Micro-level experience also provided important information for the understanding of unexpected results of the longitudinal study. For example, the hypothesized fertility differentials between the two study areas did not emerge: in fact, fertility fell almost as much in the control area than in the experimental community. On the basis of survey data alone, the author would have been hard pressed to explain this finding. However, owing to his continual contact with the area over the decade, he was able to enumerate the series of events that intervened. As we have seen, economic adjustments to development incentives and family planning initiatives were even more pronounced in the control area than in the more developed community, owing to the refusal of local farmers to stagnate under traditional productive arrangements. Lower fertility was simply another adjustment to changing economic and social circumstances and opportunities.

Thus, it may be argued that the Hackenberg study utilizes an ideal combination of methodological tools for determining the impact of development interventions upon demographic behaviour. One feels confident in the explanations provided by the analysis because they are based on various sources of data, including qualitative insights over a considerable period of time. The portrayal of regional contextual factors, including the various government programmes introduced into the region over the decade, also makes the conclusions more understandable to local and national decision makers and hence more amenable to policy implementation.

Conclusion

In conclusion, some general observations concerning the impact of development programmes upon demographic behaviour may be derived, based on the above studies and on the IDRC experience more generally. First, single theoretical models of economic or demographic responses to development are often inadequate to explain the complexity of rural behaviour in third world societies. Peasants frequently react in unanticipated ways to changing circumstances, by making adjustments which they perceive to be to their advantage which may not correspond to the responses anticipated by the development planners. The Philippine case study illustrated the resiliency of farmers to adapt not only to positive structural changes but also to potentially adverse economic conditions. The result, for the majority of residents in the more backward areas, was to turn an apparently worsening situation, landlessness, into one of relative prosperity, through the establishment of new labour arrangements.

A related conclusion emerging from this, and many other studies funded by IDRC (Aramburu, 1986; Forni and Benencia, 1986; Giraldo Samper, 1986)) is that "accidental" or unexpected factors frequently intervene to alter anticipated relationships between development inputs and demographic outcomes. This has already been discussed in some detail with respect to the Hackenberg study, but it was also evident in the Thai reforestation project with respect to the higher mortality and fertility levels in many of the reforestation villages. However, owing to the lack of in-depth qualitative information in the latter analysis, we are left with many questions concerning the nature and effects of these intervening variables.

Most studies seem to agree that areas experiencing favourable economic trends, such as the reforestation villages in Thailand, tend to retain their population more than non-programme areas. This indicates that progressive agrarian policies which provide increased employment opportunities in the agricultural sector can have a positive stabilizing effect upon rural areas. While the Hackenberg study may appear to be an exception, in that out-migration was fairly pronounced, this was due primarily to "pull" factors rather than to "push" causes, as it generally involved young adults from high income households, who left in search of greater educational and employment opportunities elsewhere. Although it is true that the local farmers adjusted their economic responses to meet the changing circumstances, they were also realistic in realizing that only a limited number of agricultural workers could be absorbed within the existing framework. They therefore encouraged their children to leave in order to diversify the family's economic base and to relieve population pressure in the rural origins.

Another important finding of the three studies is that fertility declines tend to accompany intensive development and family planning efforts. Although the results for Thailand were less consistent than might have been hoped, it appeared that the participation of women in reforestation jobs led them to reduce their fertility to some degree. In the Philippines and Bangladesh, a combination of favourable social and economic conditions seemed to make birth control a logical course of action for rural families.

A nearly universal finding of development impact studies is the marked importance attached to education by the target populations. It is seen both as a means to alter unfavourable economic circumstances, in that educated children can provide more security for their families than uneducated ones (Bangladesh), and as a way of improving already positive outlooks (Philippines). Education is also clearly related to rising age at marriage for females, lowering fertility ideals and fostering greater attention to health and care of children.

The overriding importance of economic change in determining demographic outcomes has been demonstrated by most of the research supported by IDRC. Economic circumstances affect, to a large extent, the types of social and demographic strategies adopted by rural families to survive and adapt to the modern world. Thus, any research which aims to elucidate the impact of development interventions on the demographic or other behaviour of a society must consider not only the micro-effects at the household level but also the broader economic changes taking place in the larger society, at both local and national levels.

Since many projects on the demographic consequences of development programmes have been funded by IDRC over the past several years, and most have now been completed, it is probable that support to this topic will decline in future. However, the lessons learned from these studies, particularly in terms of methodology, can also be applied to other topics. The importance of combining a number of methods, exemplified by the Hackenberg study, has been demonstrated by this research and has provided a model which is broadly applicable to a great deal of social science research.

In this mix of methods, micro-level analysis plays an important role, for the reasons emphasized in this paper. However, it must be emphasized that micro-level research is a relatively new phenomenon for demographers and that it requires training and sensitivity, just as in other disciplines with which a researcher is unfamiliar. Hence, it should not be assumed that the micro-approach can be undertaken by any given researcher or simply as an "add-on" to other investigations. The various methods should be carefully selected and integrated from the beginning of the research and carried out by qualified and trained investigators.

The observations made in this paper concerning the usefulness of a combination of methods are also relevant to several other areas of research which are of interest to IDRC. Longitudinal studies which employ surveys at two or more points in time, in conjunction with an experimental design and micro-analysis, are applicable to evaluations of the impact of health interventions, such as tropical disease control, or of family planning programmes, upon target populations. Studies of the impact of migration on communities of origin, such as those affected by heavy out-migration to urban areas or to other countries, can also benefit from such an approach. Such a mix of methods allows for the interpretation of complex interrelationships, changes and unanticipated consequences. Whenever possible, then, a combination of methods is recommended, allowing the researcher to take advantage of the positive contributions of each approach, but also to compensate for the inevitable deficiencies in the very nature of the data with which social scientists must work.

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D. Demographic impacts of development projects

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The International Labour Organisation (ILO) has recently commissioned a number of country review studies on the demographic impacts of development projects implemented at the community or area level. Such projects include the setting up of co-operatives, credit and income generating schemes for women, public works programmes, land settlements, rural electrification, irrigation, and integrated rural development programmes.

Country reviews are largely based on available information and data. They are therefore being carried out in countries such as Bangladesh, Indonesia, Mexico, the Philippines and Thailand where some experience exists with demographic impact assessment. The major objective is to put together country experiences and to draw lessons for the future. The analysis is focused at the micro-level to see how particular economic and social changes generated by specific development projects have influenced demographic behaviour. Within that framework, a number of policy-related questions are being examined. For example:

(a) Which development interventions are more likely to have major direct and/or indirect effects on demographic behaviour?

(b) Under what circumstances and for which segments of the population are those effects likely to be the greatest?

(c) How can those effects be manipulated?

(d) How best can the population objectives and/or programme components (population education and service delivery) be integrated into those types of projects?

(e) How best can the demographic impacts of development projects be measured and evaluated?

Answers to such questions are extremely valuable for planners and can possibly help to shift resources in development plans towards activities which have more favourable demographic impacts, as well as to improve the effectiveness of integrated population-development programmes through appropriate changes in project design.

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Review studies are currently being carried out, and it will be some time before their full results are available. However, preliminary analysis in some countries has already highlighted several methodological and other problems. One major problem in preparing review studies in most countries has been the lack of a sufficient number of studies focusing on the impact of development projects on demographic behaviour. This gap is largely due to the methodological difficulties involved rather than to a lack of interest in impact assessment. Second, although the analysis of the impacts at the community, household and individual level provides greater insights into the mechanisms through which development changes affect fertility than does macro-level analysis correlating aggregate changes, the methodological difficulties of conducting such an analysis are enormous. As a result, the findings of most micro-level studies are often ambiguous and their conclusions debatable. For example, a recent study in Thailand on assessment of the demographic impacts of an irrigation project has found that women living in areas without irrigation had lower fertility than those who were living in areas with small and medium size irrigation systems, and in some instances, lower fertility than those who were living in areas with large irrigation systems (Prasith-rathsint and others, 1981). Another study in Thailand has found that women who participated in cottage and home industries had higher fertility than those who did not participate (Chalamwong, 1983). A study in Bangladesh has concluded that women who are members of co-operatives have higher fertility than non-members (Mabud, 1986). Some of those results are perhaps due to inappropriate methodologies used. The emphasis in the review studies is therefore on the methodologies followed in the micro-level studies rather than on their findings in an attempt to suggest, with the advantage of hindsight, the future directions that research on demographic impacts might take in order to be more scientifically valid and more useful to policy makers.

Evaluation of demographic impacts is often difficult in practice. One possible approach that has most often been used is the "with and without" design, provided one can define and identify an appropriate control group. The other could be the use of "before and after" design, particularly if the baseline information is available. A better approach would be combining "with and without" with "before and after" - the longitudinal dimension being important for eliminating the effect of any prior differences between experimental and control groups.

No matter which approach is used, difficulties remain, and appropriate methodologies that can be more widely applied still need to be developed (see, for example, Stoeckel and Jain, 1986).

Nevertheless, certain methodological errors can be easily avoided. For example, a study of the impact of small-scale industry promotion programme in the Philippines finds that the programme does not have any significant effect on lowering fertility (Pernia, 1982). A closer examination of the methodology used by the study reveals that the lack of significant impact is perhaps due to the fact that the dependent variable used is the number of children ever born (CEB) which includes events in the past that cannot possibly be influenced by the project that started only a few years ago. This suggests

that for impact evaluation, measures must be developed to capture changes during the reference period of the study. In other words, if the project has been implemented for five years, the dependent variable must be measured in such a temporal context, perhaps allowing for some time-lag.

In general, there are four major methodological problems in demographic impact assessment. First, development projects influence demographic behaviour only indirectly through changes in variables, such as income, employment, health and education. It is often difficult therefore to trace out the demographic effects through changes in these intermediate variables. Second, so many changes take place at the same time that it is difficult to isolate changes due to a particular project or programme. Third, development projects are usually directed towards target populations, which often possess characteristics different from others. For example, a study in India has shown that villages receiving electricity tend to be larger villages with secondary schools and agricultural services, and villages near cities. Thus, to the extent that there is a selection bias in the choice of areas or households to receive a project, it is not correct to assume that demographic differences are due to the project rather than the result of other underlying differences. And, finally, short-term effects are usually different from long-term effects. In the case of migration, for example, when an individual leaves the farming household, the pressure resulting from the reduced supply of family labour may lead to an increase in the use of hired labour. But subsequently the household may adopt more labour-saving technology or may sell out a part of the land (Oberai and Singh, 1983). In a demographic impact assessment study, measured impacts will thus depend on the time frame used, and different periods of time between the initiation of projects and the time when their effects are evaluated will give different assessments of the impacts.

Despite those methodological difficulties, several studies have attempted to assess the demographic impacts of development interventions largely using "with and without" design. Their findings have highlighted a number of policy implications.

On rural electrification, a recent study carried out in Misamis Oriental in the Philippines suggests that the programme has had a significant negative effect on fertility (Herrin, 1986). But another study carried out in the Cagayan Valley in the Philippines as well as studies in many other countries, such as Bangladesh and Indonesia, have found no such impact (Hull and Hull, 1986; Harbison and Robinson, 1985). One reason suggested for the lack of impact in the Cagayan Valley is that the use of electricity other than for lighting has not been widespread enough to generate large and quantifiable impacts on fertility. This may suggest that if little productive use is made of project inputs, there is no point in searching for demographic impacts. In such a case, what is perhaps more interesting is to study variations in use of project inputs in order to determine which types of use are more likely to have favourable demographic impacts.

Studies of women's co-operatives in Bangladesh and rural job creation programmes in Thailand suggest that, among other things, the demographic impact depends on the size of development intervention (Prasith-rathsint, 1986; Mabud, 1986). Rural job creation programmes have provided very small

financial benefits to those who have participated in them. Most such programmes have provided work to rural households for up to 10 days in a year, and the participants have earned an average 61 baht a day. Women's co-operatives in Bangladesh have also raised members' income only marginally. It is hardly surprising, therefore, that those programmes have been largely ineffective in lowering fertility or in reducing the flow of out-migration from the rural areas.

One of the objectives of the irrigation project in Thailand was to reduce migration to the urban centres, particularly to Bangkok. But the evidence suggests that migration in fact increased as a result of project implementation. Normally, one would expect that the higher incomes and the increased employment opportunities associated with the irrigation project will slow down migration out of the project areas. But it all depends on who benefits from the project - whether it is the small farmers or large farmers. Moreover, irrigation projects are usually accompanied by other development inputs, such as new roads which open up the contracts with urban centres and thus facilitate migration. Further analysis is needed to determine the mechanisms through which the irrigation projects influence migration behaviour, but the findings of the study suggest that development projects would be largely ineffective in reducing migration unless the basic inequalities in income, job opportunities and social services are eliminated between rural and urban areas.

The evaluation of land settlement projects in several countries, such as Brazil, Indonesia and Somalia, shows that their impact on fertility can be pronatalist particularly where allocation of land leads to increased demand for family labour (Oberai, forthcoming). In Indonesia, this finding has important policy implications, since in national development terms the success of the transmigration programme in relieving population pressure in Java would be considerably weakened if similar pressures on resources were to develop in the receiving areas. It may suggest, therefore, that if it is desired to reduce fertility of women living in project areas, other aspects of community development would need greater attention, including an active family planning programme such as in Malaysia's land development schemes.

Analysis of the land reform programme in Thailand shows that the impact of the programme in reducing migration and fertility has been greater when other development inputs were made available in land reform areas (Prasith-rathsint, 1986). This suggests that perhaps an integrated development approach is more effective in improving living standards and fulfilling national demographic objectives than a single purpose development project. In most countries, however, land reform programmes have been largely unsuccessful in having significant demographic impacts. Without technical and financial assistance, small farmers have not been able to take full advantage of land reforms. In many countries what those reforms have done is to encourage landowners to mechanize production and shift to wage-labour. The result has been that small peasants have even lost their previous (insecure) source of subsistence altogether. For many of them, rural out-migration has been the only option (Oberai, 1987).

The need to integrate development projects with family planning projects is also suggested by the results of micro-level studies of family planning programmes. Several studies indicate that those programmes are unlikely to be successful in situations where the economic value of children is high because of limited opportunities of parents for income generation and old-age support and in situations where infant and child mortality is high because of limited access to health and sanitation services. Until economic and health changes reduce the need to produce large number of children, there is little demand for contraception.

In conclusion, the results of the studies of the demographic impacts of various types of development interventions are far from conclusive. This is both because of the methodological shortcomings of the studies and also because of the complexity of the underlying processes that are occurring. In cases where no impact is found, the reason may be the inappropriate design of the study and the measures used, the lack of time for an impact to occur, the insignificant economic impact of the project or a lack of relation between project effects and demographic behaviour. Only more careful research designs, in particular those including a longitudinal dimension, will help to disentangle project effects from the effects of other simultaneous changes and also from selection bias.

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III. METHODOLOGICAL AND MEASUREMENT ISSUES

A. Selected methodological issues in studying the effect of development projects on population

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A great deal has been written about the effect of development projects on demographic behaviour. The issue has been examined from a case study perspective; household survey data has been employed, as well as data for geographic units. Good reviews of the methodological problems are available (e.g., Stoeckel and Jain, 1985; Bilsborrow and DeLargy, 1985). An overall review of all the different approaches and their strengths and weaknesses is beyond the scope of this paper.

Given the focus of the Workshop and the study designs now under way, this paper's point of departure is the matrix of data called for in table 1 of the "General Overview" contributed by the Population Division to the present volume, along with the ancillary information requested in table 2 from the five country-consultants on the starting dates of key inputs (electrification, irrigation etc.). In short, one conceives of having data on input variables (electrification, irrigation etc.), expected socio-economic outputs (income, health), and demographic characteristics (fertility, mortality, migration) over time for a set of "experimental" and "control" groups (perhaps 40 of each). The data may thus be characterized as multiple observations over time of multiple aggregate units with differential magnitudes and timing of development inputs, and with a subset of zero inputs. Our focus will be to describe a range of analytic possibilities from these data, the advantages and limitations of each one; and then to discuss a range of general issues that apply to all analyses based on this type of data.

An obvious starting point, given the attempt to simulate experimental design, is the analysis of experiments. Accordingly the next section takes up a range of issues related to analysing experiments that are relevant to the data generated. These include analysis of the before-after, two-group design, confounding variables, and analysing measurements on more than two occasions. The presentation utilizes both "classical" analysis of variance approaches and linear models, and stresses several conditions that arise when dealing with quasi-experimental data rather than true experiments. Subsequently, we present a variety of other analytic possibilities for the data generated, including the use of standard curves, viewing units as their own control, threshold effects, and pooled cross-sections. A final section reviews and expands on the conceptual issues underlying the analysis of these data and attempts to develop some guidelines for future research.

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Analysis of experiments

Before-after, two-group design

As is well known, in simple experiments, some subjects (units) receive a treatment while some are placed in a "no treatment" or control condition. The treatment may be, for example, rural electrification, if the units are villages or clusters thereof. Bringing areas under irrigation networks is another example of application of a treatment. For the moment, let us imagine that the application of treatment (intervention) is instantaneous, that is, one that is completed quickly. (Obviously, this is not a realistic characterization of such a treatment as rural electrification, which typically takes a long time to complete: first a few households or non-household establishments in the local area in question receive connection; others join later. Eventually most households and non-household establishments have electricity. The process is completed over a period of several years.)

A common way of assessing the treatment impact is in terms of the following measure:

$$(\bar{y}_{ta} - \bar{y}_{tb}) - (\bar{y}_{ca} - \bar{y}_{cb}) \qquad (1)$$

where y's are means of observations, and the subscripts t, c, b, and a denote respectively treatment, control, and measurements before and after the application of the treatment. (The assumption that the application of treatment is instantaneous is convenient to insure that the "before" and "after" categories of observations are exhaustive and mutually exclusive. We dispense with this assumption later on.)

Before discussing its validity, it is instructive to view measure (1) from the perspective of linear models. Let G and T be two dummy variables attached to the measurements y_{GT}, G taking the value of 1 if the observation is on a treatment group member and 0 otherwise, and T taking the value 1 if the observation is an after measurement and 0 otherwise. Then the before and after observations of units in the control and treatment groups can be thought of as having the following structures:

Control; before: $y_{00k} = \beta_0 + \varepsilon_{00k}$

Treatment; before: $y_{10k} = \beta_0 + \beta_1 + \varepsilon_{10k}$

Control; after: $y_{01k} = \beta_0 + \beta_2 + \varepsilon_{01k}$

Treatment; after: $y_{11k} = \beta_0 + \beta_1 + \beta_2 + \beta_{12} + \varepsilon_{11k}$

where the first subscript of y represents group (control; treatment), the second one represents time or occasion (before; after), and the third the serial number of the individual observation within the specific group-time category. These, it may be noted, are appropriately called the observational equations. The last term in each equation is referred to as the residual or error. It is easy to see that in the particular specification given above, the mean of before measurements on the control group members is viewed as a

baseline measurement. The equations show specific additions to this baseline measurement needed to represent typical measurements in other group-time categories. Among the additive components thus introduced, we notice, β_1 common for all treatment group members, β_2 common for all after measurements, and β_{12} common for all after measurements on treatment group members. Conventionally, β_{12} is interpreted as representing the group by time interaction, reflecting the extent to which the time patterns of the group-specific means differ from one another. The magnitude, direction and significance of β_{12} is crucial in establishing the outcome of the experiment and its policy indications. The analysis of variance of the simple design described above would typically have the form shown in table 1 below.

Table 1. Analysis of variance: two-group, before-after design, with equal (=n) number of observations in each group and no loss to follow-up

Source of variation	Degrees of freedom	Mean sum of squares	F-ratio
Groups	1	GMS	F_1 = GMS/SMS
Times	1	TMS	F_2 = TMS/RMS
Interaction	1	IMS	F_3 = IMS/RMS
Subjects	2n - 2	SMS	
Residual	2n - 2	RMS	
Total	4n - 1		

In a manner similar to the effects in the observational equations, the ratios in the last column, if various conditions are met, permit one to test whether the experimental and control groups are significantly different (F_1), whether the before-and-after measures differ (F_2), and, most important, whether the time pattern of change for the two groups is significantly different (F_3). Two issues demand attention, however, before the results shown in table 1 can be interpreted. First, for purposes of tests of significance, we need to know the conditions under which the ratios (F_1, F_2, F_3) can be regarded as having F distributions with degrees of freedom suggested by the entries in the second column. Second, we need to know what in reality distinguishes the two groups from one another.

Let us discuss the latter first, leaving the former to be addressed later, following the presentation of the analysis of variance for the general set-up, which involves subjects providing observations on three or more successive

occasions. We know from the description of the design, that "treated" versus "not treated" seem to distinguish the treatment group from the control group. But the question arises as to whether that is all there is. As Cook and Campbell (1979) point out, we need to know a great deal more about the two groups, if we wish to avoid making misleading inferences from results such as those presented in table 1. More particularly, a substantial interaction effect (F_3) in table 1 can arise from factors other than the treatment, if the control and experimental groups differ on any of a number of dimensions. These are discussed below, first for quasi-experiments, where there is no random allocation; and then the threats to validity for random assignment are taken up.

It is possible that the two groups might have been non-equivalent to start with. That is, because of the differences in the internal composition of the treatment and control groups, the time pattern of mean observations would have been different in the two groups, even if the treatment was non-effective or absent. To illustrate, suppose we are interested in the effect of rural electrification on the health status of the population involved. A contrast of the treatment group with the control group in respect of health status may be a poor guide, unless it is valid to assume that the two groups were equivalent with regard to initial health status and potential changes therein if left undisturbed by any intervention. In the usual circumstances, it is impossible to guarantee that the assumptions just mentioned hold, unless there is random assignment of units. There are many factors affecting the health status of a population, some of them still unknown even to those who have been studying the topic for a long time. Given this state of affairs, it is virtually impossible to find two local areas (e.g., villages, districts) that are equivalent with respect to initial health status and all factors affecting the time pattern of changes therein. Under the circumstances, any observed non-zero interaction effect may wholly or partly be due to selection (allocation of subjects with particular traits contributing to the observed differences).

There are also problems that present themselves in quasi-experiments even when subjects (units) in the comparison groups are equivalent in the sense described above. If any of these effects plagues the various groups to differing degrees, the interpretation of the interaction effect becomes difficult. Among such problems are maturation, regression to the mean, and measurement impact.

Maturation is a problem when an observed effect might be due to the subject's (unit's) growing older, wiser, stronger, more experienced, more developed, and so on, when the experiment (project) is in progress, and when this (maturation) is not the treatment of interest. If the developmental path of the control and experimental areas differ independent of the treatment, this can affect the interaction team.

Regression to the mean is a problem, when an effect is due to the subject's being allocated to the comparison groups on the basis of traits correlated with the response variable (e.g., health status). Regression to the mean results in the above average gains (loss) over time in the response score among those subjects with unusually low (high) initial scores on the response variable. Regression is always to the mean of the population from

which the group members came. Its magnitude depends upon the difference between the mean of the group chosen and the mean of the population of which it (the group) is a part. Another factor affecting regression to the mean is test-retest reliability of the measure (of response) one uses in the investigation. The higher the reliability, the lower the threat due to regression. Similarly, the smaller the difference between the means of a group chosen and the mean of the population whence it was chosen, the lower the threat of regression.

As for measurement impact, the concern is whether the act of obtaining the measurement of interest in and of itself induces change in what is being measured differentially between control and experimental areas. Thus, questions about health status may make respondents more health-conscious and consequently adopt behavioural changes that affect their health.

The problems mentioned so far are believed to be of concern only in quasi-experiments, where subjects are not assigned to comparison groups at random. But true experiments (in which randomization governs the allocation of subjects to comparison groups) share with quasi-experiments the following threats to validity. Something other than the application of the treatment might have happened during the period of observation, affecting the treatment and control groups differently. In laboratory research, such eventualities are guarded by insulating participants from extraneous influences (e.g., keeping the room free of disturbing noise from outside), but in field experiments, it is much more difficult to control exogenous factors that may be differential across areas and which may affect the variables of interest.

It is also possible that the conduct of the experiment will interact with the maturation process or the impact of measurement so as to distort the estimate of experimental effect. For example, the presence of an irrigation project may mobilize a community to undertake other developmental activities which affect their income or health status, or the presence of a water project may make residents more conscious of health, so that they respond differently to questions on this topic than residents of the control areas.

In field experiments, it is not unlikely that subjects assigned to the control group become indirectly (e.g., through diffusion) beneficiaries (or victims) of the treatment applied in the treatment group. If this happens, the distinction between the comparison groups becomes blurred. It may also happen that subjects in the control group, who are denied the experimental treatment, are compensated for in some other fashion. For example, based on political or other considerations, local areas not selected for rural electrification may be allocated extra funds for strengthening public health services. If this happens, the health status of those in the control group might be higher than it would have been otherwise, and the contrasts between the treatment and control groups no longer remains a contrast of "treated" versus "not treated" conditions, but becomes a contrast of two treatments (rural electrification versus extra strengthening of public health services).

Finally, there is a possibility that subjects in the control group might deliberately seek and gain some of the same benefits as those the treatment is expected to provide to the treatment group, but by means other than the application of the treatment involved.

The investigator is obligated to ascertain through careful examination of the data whether any such problem is a cause for concern. Only when they are all ruled out, would it be defensible to interpret a significant difference between the treatment and control groups as reflecting a corresponding difference between "treated" versus "not treated" conditions. If one or more of the problems are suspected to be present, or the presence of a problem cannot be ruled out because of the lack of data or other reasons, then the possibility that the treatment impact is confounded with the impact of the problematic factor or circumstance is to be recognized and reported.

Confounding variables

As indicated above, some of the problems with respect to interpretation of results can be avoided, if subjects (units) are assigned to comparison groups at random. But rarely indeed in a society are valuable resources allocated at random to projects at the national or subnational level. The most common practice is to allocate resources on the basis of need, merit, political considerations and the like. The usual set-up therefore is one in which the values of the response variable y are influenced by a number of confounding variables, say, $x_1, \ldots, x_q$.

Confounding variables may have two effects on the comparison of the response y in two comparison groups. The groups may differ in composition with respect to one or more confounding variables. Even when the groups are similar in respect to x_i-composition, they may not be similar with respect to variability in x_i.

Consider the simple case of one time observation involving one confounding variable x. Suppose that x is linearly related to the response variable y. For the treatment group, we have

$$y_{ti} = \alpha + \delta + \beta x_{ti} + \varepsilon_{ti}$$

and for the control group

$$y_{ci} = \alpha + \beta x_{ci} + \varepsilon_{ci}$$

where δ is the treatment effect. It is easily seen that

$$\bar{d} = \bar{y}_{t+} - \bar{y}_{c+} = \delta + \beta(\bar{x}_{t+} - \bar{x}_{c+}) + (\bar{\varepsilon}_{t+} - \varepsilon_{c+})$$

If we assume that $(\bar{\varepsilon}_{t+} - \bar{\varepsilon}_{c+})$ is zero on average, it follows that

$$(\bar{y}_{t+} - \bar{y}_{c+})$$

is an estimate of

$$\delta + \beta(\mu_{t+} - \mu_{c+})$$

where μ_{t+} and μ_{c+} are respectively the population means of $\bar{x}_{t+}$ and $\bar{x}_{c+}$. We immediately notice that the bias in using $(\bar{y}_{t+} - \bar{y}_{c+})$ as an estimate of δ is $\beta(\mu_{t+} - \mu_{c+})$. Further, irrespective of whether $\mu_{t+} = \mu_{c+}$, we have, assuming that variances of x and ε do not differ across comparison groups, the variance of $\bar{d}$ given by

$$(2/n)\ (\beta^2\ \sigma_x{}^2 + \alpha_\varepsilon{}^2)$$

which can be written as

$$(2/n)\ [\rho^2 \sigma_y{}^2 + (1 - \rho^2)\sigma_y{}^2],$$

where ρ is the correlation between x and y.

It follows that if the effects of variation in x could be removed, or adjusted for, we could reduce the variance of $\bar{d}$ from $\frac{2}{n}\ \sigma\ \frac{2}{y}$ to $\frac{2}{n}\ (1-p^2)\ \sigma_y{}^2$. In simple controlled experiments, because of procedures such as randomization, the investigator does not have to worry about bias in using $\bar{d}$ as an estimate of treatment impact. Even then it may be worthwhile to control for the confounding variable x for the potential increase in the precision of $\bar{d}$.

There are two ways to control for the variation in the confounding variable x. If the linear regression model is correct, selection of the treatment group and control group so as to make $\bar{x}_t$ and $\bar{x}_c$ as nearly equal as possible would reduce the danger of bias and increase the precision in $\bar{d}$. Another way of accomplishing these objectives is to adjust for x at the analysis state, by taking for an estimate of the treatment impact

$$\bar{d}' = \bar{d} - \hat{\beta}(\bar{x}_{t+} - \bar{x}_{c+})$$

where $\hat{\beta}$ is an unbiased estimate of β. Extension of these ideas to before-after, two-group designs is straightforward.

Analysis of measurements on more than two occasions

Let us shift attention to the case in which measurements are available on, say, t occasions on subjects allocated to the treatment and control groups. Let y_{0ij} stand for the observation of the ith subject in the control group at at the jth occasion, and let y_{1ij} stand for the observation on the ith subject in the treatment group at the jth occasion. Let us further assume that there are n subjects in the control groups and an equal number in the treatment group. Furthermore, let there be no loss to follow-up. Under this setup, the analysis of variance of the data takes the form as shown in table 2.

Table 2. Analysis of variance: two-group design, with observations on t occasions; balanced case, involving n subjects in each group and no loss to follow-up

Source of variation	Degrees of freedom	Sum of squares	Mean SS	F-ratio
Group	1	$t\sum n(\bar{y}_{g++} - \bar{y}_{+++})^2$	GMS	F_1=GMS/SMS
Occasions	t - 1	$ng\sum(\bar{y}_{++j} - \bar{y}_{+++})^2$	TMS	F_2=TMS/RMS
Interaction	t - 1	$n\sum\sum(\bar{y}_{g+j} - \bar{y}_{g++} - \bar{y}_{++j} + \bar{y}_{+++})^2$	IMS	F_3=IMS/RMS
Subjects	2n - 2	$t\sum\sum(\bar{y}_{gi+} - \bar{y}_{g++})^2$	SMS	
Residual	(2n-2)(t-1)		RMS	
Total	2nt - 1	$\sum\sum\sum(y_{gij} - \bar{y}_{+++})^2$		

The sum of squares for occasions being the sum of squared differences

$$(\bar{y}_{++j} - \bar{y}_{+++})^2$$

indicates whether the mean level of response varies over time. The interaction sum of squares indicates whether group-specific patterns of variation over time differ from one another.

The ratios F_2 = TMS/RMS and F_3 = IMS/RMS do not have F distributions with degrees of freedom suggested by the entries of column 2 of table 2, unless certain conditions hold. One such set of conditions is that the standard deviations are time and group invariant and the serial correlations of observations are lag and group invariant (Huynh and Feldt, 1970). These conditions seldom hold in practice. The most common pattern is for the correlations to decrease with lag.

Box (1954) has suggested that one could use F tests if the degrees of freedom are adjusted for the correlations over time. He has derived a multiplier for the adjustment of the degrees of freedom. The multiplier attains its maximum value unity when the correlations are lag invariant. More restricted set-ups, such as when the serial correlations between observations u time units apart is ρ^u, have been investigated by Hearne and others (1983), and Wallenstein and Fleiss (1979).

If one relies on F tests guided by the entries in the second column of table 2 for fixing the degrees of freedom, the chances are that in most practical situations the null hypothesis will be rejected more frequently than warranted, suggesting experimental effects where non exists.

Use of orthogonal polynomials

When successive occasions are evenly spread over time, one may decompose the sum of squares due to occasions into linear, quadratic, cubic etc. components. Suppose, for example, that four occasions are involved. Then we can form a linear, a quadratic, and a cubic component for time pattern of observations on each subject, by combining the four successive observations using the following weights:

	Linear	Quadratic	Cubic
Occasion 1	-3	1	-1
Occasion 2	-1	-1	3
Occasion 3	1	-1	-3
Occasion 4	3	1	1

(These numbers are taken from tables of orthogonal polynomials (see, e.g., Fisher and Yates (1943, table 23). Snedecor and Cochran (1980, pp.404-407) provide illustrative use of these numbers. Note that when there are t occasions, we use t-1 sets of numbers so as to obtain that many linear combinations.) Linear combinations formed using these weights are non-correlated with each other. The contribution to the sum of squares from a weighted sum $\sum\lambda_i y_i$ where λ_i's are weights mentioned above, with $\sum\lambda_i = 0$, is

$$(\sum\lambda_i y_{++j})^2/(ng\sum\lambda_i^2)$$

The sums of squares thus computed using the successive sets of weights add up to the sum of squares due to occasion. The advantage of decomposing the occasion sum of squares in this fashion is that often higher-order components turn out to be not significant, in which case, parsimonious representation of the time pattern becomes possible. Corresponding to each component of the occasion sum of squares there is a component of the interaction sum of squares with k - 1 degrees of freedom, k being the number of groups involved.

Utilizing the pattern of change in the individual units

In this section we briefly describe several approaches that take account of the fact that a series of observations is available for each unit and which utilize these patterns to make inferences about the effect of projects. These approaches involve the use of curves to represent time patterns, viewing each unit as its own control, and testing for the existence of threshold effects.

Use of standard curves to represent time patterns

An exploratory plot of each unit over time (or of the mean observations per group) on an outcome measure of interest may reveal that the observations fit a common shape (e.g., linear, logistic etc.). If so, one can express this pattern in terms of the parameters of the curve (one or more depending on the shape) for each unit, and then seek to explain the magnitude of the parameters in terms of development inputs or intervening socio-economic factors, using appropriately lagged magnitudes.

To illustrate with a simple case, assume that birth rates are declining in a linear fashion in each of the control and experimental units. The slope (negative in this case) can be calculated for each unit and as a first approximation, one can test if the speed of decline is greater among units that have received developmental inputs than those that have not. More precisely, one would test if the magnitude of the negative slopes is greater among units with larger inputs, assuming one has a measure of inputs suitably prior to the period of observations. Since the developmental inputs are likely to occur within the period of observation, one may want to measure for the experimental units, the slope prior to the inputs and the slope subsequent, and test that the rate of decline is sharper in the latter period. A similar approach of comparing trend lines before and after the introduction of a family planning programme is a well-known technique for assessing the effects of such programmes (see United Nations, 1979, pp. 149-152).

Postulating different parameters for different segments of the time interval, also includes the possibility that the shape of the curve will change over time, so that it is necessary to fit the models piecewise. Sometimes response to treatment follows one pattern for a period of time and another afterwards.

Often one will want to go beyond a simple linear trend. Changes in a number of demographic phenomena of interest are often a sigmoid type, moving from an initial asymptote in a curvilinear manner to a lower or higher asymptote. Curves of this nature may be captured by fitting a logistic or

Gompertz curve. Anderson (1974) used a logistic curve to trace the change in marital fertility over time among the cities and counties of Taiwan, and then used characteristics of these units to explain four parameters of the curve - the upper and lower asymptote, the date of onset of the decline, and the rate of decline. A similar analysis focusing on nuptiality change was carried out by Casterline (1980).

Viewing a unit as its own control

Another way to make use of the over time data for each unit is to statistically analyse the time series to test whether the outcome variable is different before and after the intervention. Imagine an intervention programme, e.g., the introduction of a land reform; the starting of a rural credit society; making available to a village water from a nearby irrigation network; and so on. Let us assume for the moment that the intervention is instantaneous. It would be inappropriate to apply a t test to the difference in means of a variable of interest, say, y, before and after the date of intervention because of the lack of independence. How else can one test the effect of intervention on y? We start by writing

$$y_t = \beta x_t + z_t \qquad (2)$$

where y_t is the value of y at time t, x_t has the value 0 if t denotes a date prior to the intervention date and the value 1 otherwise, and z_t is a residual term. In this set up, B will represent the possible shift in the level of y that occurred as a result of intervention. In practice z_t's are likely to be correlated. Suppose we suspect that correlations one lag apart are significant while those with greater lags are negligible. To specify a model that incorporates these features, we may proceed as follows: Let ε_1, ε_2, etc. be a sequence of random variables, each with mean (expectation) zero and variance σ^2_ε and independent of each other. It is easy to see that if we write

$$z_{t-h} = \varepsilon_{t-h} - \theta\varepsilon_{t-h-1},\ h = 0, 1, 2, \ldots \qquad (3)$$

then z_t will be correlated with z_{t-1} (because both contain the random variable ε_{t-1}), but z_t will not be correlated with z_{t-h}, for $h = 2, 3, \ldots$, because z_t and z_{t-h} do not have any ε's in common, and all ε's involved in them are independent of each other. We thus have

$$y_t = \beta x_t + \varepsilon_t - \theta\varepsilon_{t-1} \qquad (4)$$

Whether this is the specification appropriate for a specific problem on hand can only be determined by examining the data. But the specification can be easily modified, if necessary. For example, instead of x_t we may use a pair of dummy variables, say, x_{t1} and x_{t2}, if we wish to segment the time dimension into three parts, instead of into two as was suggested above:

	x_{t1}	x_{t2}
Prior to the start of the intervention programme	0	0
Between the start and the completion of the programme	1	0
After the completion of the programme	0	1

This type of segmentation of the time dimension is appropriate for intervention programmes that require a certain length of time to complete. Another type of modification that may be appropriate is to introduce on the right-hand side of (4) other exogenous time series whose influence needs to be taken into account. The decision as to what exogenous time series should be incorporated should be based on substantive knowledge regarding the dynamics under investigation. Yet another modification may be to posit that serial correlations in the z sequence of lag greater than 1 may be non-zero. Such a modification may be necessary, for example, if allowance is to be made for seasonal characteristics of the data.

Methods are available for fitting models of the type mentioned above and for submitting the fitted model to a process of diagnostic checking in which the residuals (the estimated ε's) are examined to ensure that the model is adequate (see, e.g., Box and Jenkins, 1970).

Study of threshold effects

A persistent theme in demographic transition is the idea of a critical threshold. "Improving economic and social conditions are likely to have little if any effect on fertility until a certain economic and social level is reached, but once that level is achieved fertility is likely to enter a decided decline and to continue downward until it is stabilized on a much lower plane" (United Nations, 1955). This hypothesis has been discussed and analysed by several analysts (for a recent review which cites much of the previous literature, see Cutright and Smith, 1986).

The foregoing sections on piecewise curve fitting to the observed data, and to the use of experimental units as their own controls are both consonant with the existence of thresholds but do not postulate their existence.

The matrix of data generated by the study design can be used to examine the existence of various types of thresholds. Following Srikantan (1977), the communities at the start of the period of observation can be divided a priori into low, high, or "in between," on fertility level, and for each community, the level of the associated socio-economic or development characteristic is noted. The existence of a threshold is indicated by the fact that the high and low fertility communities show little overlap in the associated characteristics; for example, the highest life expectancy for a high fertility community is lower than the lowest life expectancy in a low fertility community. (More formally, one can derive a "separation count" and a "coefficient of separation" based on the number of paired comparisons between the two groups and the number of them that conform with the hypothesis of separation.) This type of test can be carried out for a wide variety of characteristics thought to be associated with fertility change (or with other demographic behaviour that is likely to respond to threshold levels).

Given that the communities are observed over time, one can determine the extent to which the thresholds established at the start of the period are confirmed at a later date. This is, do communities that reach the hypothesized threshold level on a given socio-economic measure achieve the level of fertility associated with the low fertility group. From the

standpoint of understanding the role of development, it is also of interest to study the level of input required to achieve a threshold level of a socio-economic characteristic. Was there a strong relation between the two, or were threshold levels reached as often in the "control" areas as in the "experimental" areas?

Pooled cross sections of inputs and outputs

An analytic approach that can incorporate many of the features of the foregoing techniques and which makes maximum use of the data matrix is that of pooled cross-sectional and time series data. The study design calls for repeated measurements of a number of control and experimental units. These measurements can be viewed as replicating a time series of t observations over n units or, alternatively, repeating cross-sectional information on n units for t time points. Pooling these data points produces nt observations which can be used as the unit of analysis in a multivariate analysis. Depending on the questions posed, it may be desirable to use the data from both the experimental and control units in a single analysis, or it may be desirable to examine the experimental units alone.

As illustration, one may wish to examine the relation between a demographic outcome and a number of social and economic determinants. In this case, one can write:

$$D_{it} = \mu_0 + \beta_1 X_{i(t-5)} + \gamma Z - \alpha_t - \theta_i + \varepsilon_{it} \qquad (5)$$

where i represents the areal units

t represents the time points

D_{it} represents the demographic characteristic of unit i at time t

β_1 is the regression coefficient

$X_{i(t-5)}$ is the socio-economic factor (say income) at time (t-5); obviously the equation can incorporate a number of socio-economic variables and the appropriate time lag should be determined from theoretical considerations as well as empirical tests.

γ represents the effect of membership in the experimental group

Z is a dummy variable which takes the value 0 if the unit is in the control group and 1 if it is in the experimental group; in so far as development projects can take place at different times over the period of observation, Z can be made variable over units and time.

α_t is the effect associated with a specific time point

θ_i is the effect associated with a specific areal unit

ε_{it} is the residual for unit i at time t

Several features of the equation should be noted. Models which utilize pooled cross-sections are apt to be misspecified unless several cautions are observed (see Dielman, 1983). Following Cutright and Smith (1986), the model allows for period specific effects and community specific effects to take account of the likelihood of correlated disturbances within units over time, and across units within given time periods. (Other approaches are possible, as discussed by Dielman, (1983).)

The equation as given assumes that the effect of the socio-economic determinants, as reflected in β_1, is constant over time. If this is not tenable from a theoretical standpoint or on the basis of empirical exploration, interaction terms combining time and the socio-economic characteristic can be added.

Another feature of the equation is the assumption that whether a unit is in the control or experimental group has a direct effect (given by γ) on the demographic outcome. This can be modified in two ways. One one hand, a model which holds that development projects have direct effects only on intermediate characteristics, like health or income, would exclude group membership from the equation. Alternatively, one may propose that group membership interacts with the intermediate variables, so that the effect of a given level of income is different for a unit in the experimental group than for one in the control group. In this case, one would add interaction terms between group membership and the socio-economic effects to capture this hypothesized pattern.

As the foregoing suggests, to fully capture the hypothesized model may require a system of equations. For example, one may wish to test whether developmental inputs do affect the intermediate health and income variables. In this case, one might introduce equations of the form:

$$X_{it} = \mu_0 + \delta_1 I_{t-5} + \phi_k Z_{itj}$$

$$\alpha_t + \theta_i + \varepsilon_{it}$$

where X_{it} is the health or income value of unit i at time t; I_{t-5} represents the developmental input, suitably lagged, with an effect δ_1; Z_{itk} represents an array of determinants of X_{it} with effects ϕ_k; and, as above, α_t and θ_i represent time and unit specific effects. Consideration should be given whether equations of this type should be estimated for the combined control and experimental units or simply for the experimental units. (The absence of inputs for the control group units will produce an unusual distribution for I_{t-5}). A related strategy would be to estimate a difference equation over several points, to test whether the change in the intermediate health or income variables are associated with the inputs, suitably lagged.

The analysis of pooled cross-sectional and time series data can incorporate features of several of the techniques discussed earlier. For example, the equation set forth in viewing a unit as its own control can be seen as an aggregation across the experimental units of an equation such as

$$D_{it} = \mu_0 + \beta_1 Z_{1it} + \beta_2 Z_{2it} + \alpha_t + \theta_i + \varepsilon_{it} \quad (7)$$

where $Z_{1it} = 1$ if unit i is in the midst of an intervention programme at time t and 0 otherwise; and $Z_{2it} = 1$ if unit i has completed a project at time t, and 0 otherwise; and the other units are as defined previously. In this formulation, β_1 and β_2 measure the effect of each stage of the programme intervention relative to μ_0, prior to any programme inputs. The conditions under which one can aggregate across units are discussed by Dielman (1983).

The pooled cross-sectional and time series data can also be tested for the presence of thresholds. If, for example, in equation (5), it is posited that $X_{i(t-5)}$ has a different effect on D_{it} beyond a certain magnitude, this can be incorporated by adding to the equation additional terms to represent the change in impact of the coefficient (as well as the intercept) in the specified range. Cutright and Smith (1986) used this approach with country data pooled over time to allow the effect of expectation of life on the crude birth rate to differ when the expectation was below 46 years, 46 to 56, and 56 and above.

The ability to utilize the pooled cross-sectional and time series data to test a variety of substantively interesting models makes it a versatile tool for analysing the impact of development for data generated by the current study design.

Conceptual issues related to the demographic impact of development projects

In this section we address a number of issues that arise in tracing the impact of development projects on demographic outcomes. The issues selected revolve around the question of theory - how can we achieve better understanding of the processes generated by a planned intervention? or, correspondingly, how can we safeguard that our ignorance of the mechanisms does not distort the inferences derived from analysis? The starting point is again the difference between a "true" experiment and a "quasi"-experiment; this is followed by a more general discussion of the various aspects of theory which require further attention.

"True" experiments versus "quasi" experiments

A number of the cautions that must be observed to correctly infer that a treatment had an effect were discussed above. The key concern is selectivity - how can one insure that the experimental and control areas are comparable in all regards except the treatment. The only safeguard is random assignment, and, as noted, this is rarely possible. Consequently, it is common practice to match the control groups to the experimental units on a range of socio-economic characteristics. Often overlooked in this process is a determination of how the developmental inputs were allocated. Why did some communities obtain electricity and others not; or irrigation, or schools? In some cases, allocation may be mainly ecological (nearness to the existing grid of service, or appropriate infrastructure to accommodate the new input); in

others it may be more political (ability to mobilize resources and influence decision-makers); and in still others the units may be selected because of their extreme values on a distribution (very low income, or very high fertility). The method of selection is likely to be related to the demographic and intermediate variables of interest and their trends, in ways that might not be captured by the more usual "control" variables. For this reason, it is important to determine how developmental projects were allocated to specific areas and to the extent possible to incorporate this information in selecting control units.

Beyond controlling for factors that may lie behind the allocation of inputs, there is the broader issue of the universe from which the control units should be selected. In some cases, the experimental units are compared with data for the rest of the country; in other cases, areas adjacent to the experimental units are selected in an attempt to control for local factors. It is doubtful that hard and fast rules can be established, but it is likely that something in between these two extremes will be optimum: selection from the same region or province will control for a wide array of local characteristics (religion, ethnicity and culture are likely to be similar), but requiring some physical separation will avoid the spill-over and diffusion effects that are likely to occur over time between two units that are in regular contact.

What is learned from a "true" experiment

It was pointed out earlier that even in a true experiment, threats to valid inference exist from several quarters - uncontrolled exogenous forces, contamination between experimental and control units etc. Here we stress the connections between experiments and theory.

Assume that a careful "true" experiment determines that there is a significant difference in the change in fertility between the experimental and control areas. This alone tells us nothing about the mechanisms by which the change was brought about. Underlying theory may assume that this arose from changes in income or health, but unless one tests for significant changes in these intermediate variables, the experiment does not confirm this model. For example, the change in fertility may arise because the presence of the developmental input (e.g., an irrigation project) led to greater mobilization of community activity through the creation of co-operatives, associations etc., and these organizations became the focal point for diffusing new ideas about family planning. This competing hypothesis can also be tested as part of the experiment, but only if it is set forth explicitly as a hypothesis to be addressed. In short, the experiment will not produce theory but help test ideas derived from theory. For this reason, it is important to think broadly and deeply about the theoretical structure before deciding on what is to be measured.

In a similar vein, it must be recognized that the theoretical structure should address the issue of the time lag between the developmental inputs and the demographic effects. A priori judgements are needed to insure that the experiment will be properly designed to observe the hypothesized effects. In some cases experiments appear to fail because one does not allow for a period

of observation long enough for the presumed effect to occur. (It is also possible that short-term transitory effects will be missed when the period of observation is too long.) In this regard, note should be taken that the time period between developmental and demographic change is usually viewed as comprising two distinct periods: the gestational period between the developmental input and productivity changes (i.e., gains in income, health etc.); and the gestational period before changes in productivity are translated into changes in demographic behaviour. Experiments that seek to establish both effects must design their periods of observation accordingly.

Level of analysis - aggregate versus individual

The data matrix under discussion is based on areal units. In so far as the underlying theory assumes that demographic changes come about by the behaviour of households who are the beneficiaries of the developmental inputs, none of the analytic techniques described can test this. In order to avoid the ecological fallacy data must be collected on the household level and show that the relationships are as expected. This of course presumes a sufficiently well developed theory so that the mechanisms can be identified and measured. At the same time, the possibility must not be overlooked that there are true community effects - changes in the infrastructure due to a developmental project which benefit all the members of the community, regardless of their own participation in or direct from the project.

A strategy for combining data for individual (or households) and communities through multilevel analysis appears to be promising for the evaluation of family planning and health programmes, and may hold similar promise for studying the impact of development projects (Hermalin, 1986). In this regard it may be useful to capitalize on the existence of communities with and without development projects in designing future demographic surveys. That is, the presence or absence of a development project can be one basis for stratifying the sampling areas to ensure that these will be representative households from both types of communities. This would greatly enrich the data base for future studies of the relation between development and various outcomes.

Conclusion: understanding the dynamics of demographic change

As touched on in the previous sections, the existence of adequate theory is crucial. Without a well developed theory that specifies the mechanisms by whih developmental inputs get translated into demographic change, it will not be possible to know why some inputs have effects and others do not (or why the same input works in some places but not in others), nor to understand who those who do not directly benefit from a developmental change behave similarly to those who do.

A well articulated theory would specify the chain of events at both the community and household levels through which a developmental input gets translated into a specific demographic change, and indicate the time period over which the effects operate.

The situation is even more complex when one considers the potential interrelationships among the demographic outcomes. Changes in mortality may affect migration (by household density, inheritance patterns etc.) as well as fertility; indeed, the three primary processes are all potential causes and effects of each other. In addition, one must give attention to other demographic processes that can accentuate or mitigate measures of mortality, migration or fertility, such as changes in patterns of nuptiality or breast-feeding.

Attention must also be given to the appropriate measure of the demographic process of interest. For example, overall levels of mortality among young adults may change less than the specific causes of death, since there may be trade-offs between increases in motor vehicle and accidental deaths with certain kinds of development, and decreases in infectious diseases due to improved medical care.

To summarize, a well-articulated theory relating sectoral developmental projects to demographic change would spell out the specific processes through which both community change and household decision-making impinge on each type of demographic behaviour, and also take into account the interrelationships among the demographic factors. From a policy standpoint, such an understanding provides the only assurances that the major factors affecting demographic change will be incorporated into the planning of developmental projects, as well as providing the most efficient means for attaining desired demographic ends.

At the same time it must be recognized that planning for developmental projects must proceed in the face of imperfect theory. To this end the analyses of data matrices of the type proposed, across different projects and countries, can identify the major associations between specific projects and demographic change, thereby contributing to the planning process and providing the bases for improved theory by well-designed supplementary data collection.

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B. Data and measurement problems in aggregate analysis

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Aggregate level demographic analysis has a long and rich theoretical history. 1/ Without reviewing that history here, it should be emphasized that, throughout most of its development, social scientists have essentially worked from two, not necessarily mutually exclusive, concepts of population. In the first of these perspectives, populations are viewed as closed systems containing all the elements necessary to explain any change which occurs in the system. This concept of population is best illustrated by means of the traditional demographic equation

$$C = P_1 - P_2 = P_1 + B_{1-2} - D_{1-2} + M_{1-2}$$

where C is the change which occurs between two points in time; P is population size; B, births; D, deaths; and M, migration. The subscripts designate points in time.

As generally employed in demographic analysis, this concept of population has four structural dimensions. These are size, age structure, sex structure and distribution in social or geographic space. Theoretically, these structural properties are dimensions of the aggregate and attention is focused on these and changes in them, although it is also easy and frequently convenient to decompose the total aggregate and examine change in terms of specific categories of one or several of the structural dimensions such as age or age and sex.

From this particular perspective, all change occurring in the system is the product of system (or demographic) processes (i.e., fertility, mortality or migration). The limits and conditions under which these processes operate, in turn, are set by the structural dimensions of the population. Demographic processes are not viewed in terms of the behaviour of individual actors, but rather as attributes or characteristics of the population. 2/

This perspective of population largely has its roots in stable population theory. 3/ It has been employed extensively in demographic analysis, and we have learned a great deal about population change from it. The important point, in terms of our purposes here, is that this concept of population is a purely aggregate one, which lends itself nicely to monitoring through conventional demographic measures both in terms of its structures and processes.

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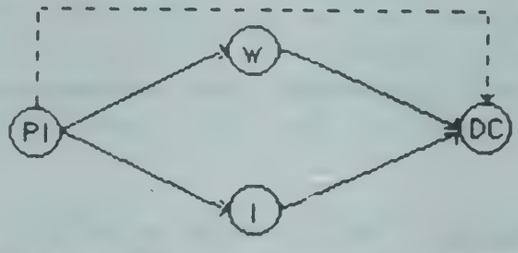
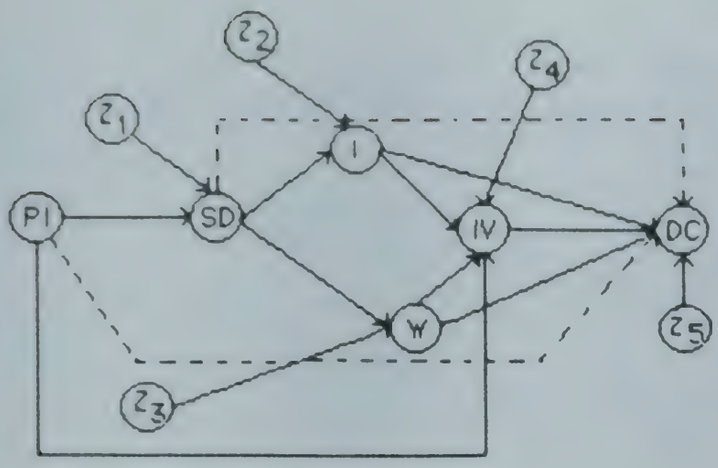
However, the aggregate perspective is composed of the sum total of demographic events at the individual level. 4/ The structural dimensions of population are seen then as products of the behaviour of individual actors and the demographic processes are thought of as summations of the behaviour of individuals in the system. Thus, from the micro-concept, attempts to explain population change take the form of examining the behaviours of individual actors and changes in those behaviours. While the units of analysis may be individuals, it is important to note that analyses employing this perspective are not always limited to the same individuals through time. In some instances, attention may be focused on how much change has occurred in a given level of demographic behaviour between individuals who pass through the same ages at different points in time. From this perspective, explanations of population change are grounded in explaining changes in individual demographic behaviour.

What has been described so far are not "theories of population", but rather the basic concepts of population employed in most "theories of population". The theories only come forth when other explanatory and intermediate concepts are causally linked to the population concept. At the simplest level, the general theoretical model that I have been asked to consider views demographic consequences as a result of only three variables. These are illustrated below in a relatively simple diagram where PI is a project input; W, welfare; I, income; and DC, some demographic consequence of the project input.



The underlying theory here is that demographic change is directly effected by changes in income and welfare and that development project inputs have no direct effects on demographic change, but rather that they influence demographic change through their impact on income and welfare.

The problem, however, even in its minimum form (where one wants to accept the basic assumption that income and welfare are the primary variables affecting demographic change) is that two additional considerations must be included. These are represented in the diagram below by SD and IV; where the former represents the structural conditions that directly determine the income and welfare in a population and the latter concerns those factors which intervene in the relationship between income and welfare on the one hand and demographic change on the other.



That is, although this latter diagram has only two additional considerations added, it creates a considerably more complicated, but theoretically more realistic research model.

This results from the fact that the model readily acknowledges that most programme inputs do not directly raise income or increase welfare without altering the structural conditions that have determined income and welfare in the past. For example, an electrification programme is not likely to produce any change in the income level of a population until the electricity is put to use to either diversify the local economy or to expand production by increasing the efficiency of the existing productive base. In a similar manner, there are a number of factors which may intervene in the relationship between a particular demographic consequence and a change in the income or welfare level of the population. A readily apparent example here, for instance, might be where a rise in the income potential resulting from a programme input, and the structural changes which it brings about alters household composition and, in turn, could have an independent effect on demographic change.

Finally, the diagram also helps to illustrate three additional points which are of critical importance in most aggregate analyses. First, programme inputs can affect demographic change either directly or indirectly. And, it is equally, if not more, important that we both measure and understand the indirect effects as well as the direct effects. Secondly, a number of factors (represented by the Z's in the diagram), external to those under consideration in any particular situation, may influence a variable in the system in addition to those actually measured. Thus, for example, a programme input may influence the structural determinants of income. But, these may also be influenced by other government policies quite independent of the programme input. This problem becomes somewhat more complicated as we move from left to right in the causal (diagram) sequence in the sense that each additional variable can be effected in the same manner.

Lastly, the diagram clearly implies a temporal sequence, but tells us nothing about the metrics of time involved and how we should expect them to operate. No theory of which I am aware tells us how long it should take for a programme input to bring about a change in the structural determinants of income or welfare. Or, if it should take more or less time for income or welfare to have an effect on demographic change than it takes for the programme input to have its effect on income or welfare. Similarly, we have remarkably little understanding of how direct and indirect effects are related in temporal sequences. Most researchers expect direct effects to materialize (or at least they look for them) more quickly than indirect effects. But it is just as important to consider the possibility that the relationship between direct and indirect effects may change over time.

Finally, it is important that a word of caution be inserted concerning the interpretation of data and levels of analysis. The two concepts of population discussed above draw a clear distinction between macro- and micro-levels of analysis. The aggregate concept of population is concerned with processes and structures and changes of variations in these. While it is possible to view these structures and processes as nothing more than the aggregate of individual behaviour, it is important to bear in mind that individual level inferences cannot be drawn from aggregate level analyses. This results from the fact that the measurement of phenomena at the aggregate level represents a measure of central tendency and does not tell us anything about how individual behaviour may vary within the population being observed. For example, to assume that a correlation between a rise in an aggregate measure of income and a decrease in an aggregate measure of fertility can be interpreted to mean that a rise in individual income will reduce the number of children that each person will have is to commit the error known as the ecological fallacy. 5/ If one wishes to make inferences about individual level behaviour, it is important to bear in mind that individual level measurement is necessary. In the same way, it is important to bear in mind in conducting individual level analyses that decisions need to be analysed separately from acts. That is, at the individual level, one should not make inferences about decisions from data which measure actions. 6/

While none of these issues concerning levels of analysis and interpretation can be dealt with in total in this paper, each is vitally important to bear in mind as a theoretical constraint governed by the level of measurement at which the researcher is working. Ideally, we would prefer that micro- and macro-level analyses not be conducted mutually exclusive of one another, but rather that macro-level factors be considered as contexts within which micro-level observations occur; but this is not always possible. 7/

The general model and the aggregate concept of population outlined above can be used to consider some of the issues researchers are likely to encounter when attempting to employ aggregate level analyses to assess the demographic consequences of development programme inputs.

Sources of aggregate data and their constraints

In most countries of the world, the most readily available source of information on aggregate demographic structures and processes is the population census. Population censuses are particularly valuable sources of information because they theoretically contain a count of the total population at some clearly specified point in time. In addition to this, censuses also include information on the age and sex structure of the population. And they quite frequently contain information useful in assessing levels of mortality, fertility and migration as well as data depicting the social and economic characteristics of populations. 8/

While censuses can be a potent source of information, it is particularly important that the researcher utilize data from them only after they have a full understanding of some of the basic parameters constraining their use. This is particularly true when census data are to be used at a sub-national level or when researchers must compare data from one census with data from a second census, as is likely to be the case when we are concerned with the demographic consequences of development programme inputs.

The first set of factors one needs to consider in this sense relates to the type of count obtained in the census and the extent to which the coverage can be considered biased. Censuses generally attempt to count individuals either where they are at the time of the census (a de facto count) or according to their usual place of residence (a de jure count). Many more countries now employ de facto rather than de jure counts. And this raises the distinct possibility that the counts for many local areas do not accurately reflect the "true" count of persons who reside in those areas. That is, while de facto counts remove the often difficult task of defining "usual place of residence", they result in "over-enumerations" for places where people are more likely to be visiting, and "under-enumerations" for the places from which the visitors depart. This can have particularly important consequences for the calculation of vital rates as well as for the simple calculation of population change between two (census) points in time.

Moreover, we should note that de facto counts make the availability of family statistics far more difficult since family members may be enumerated in different places. And that they also make the calculation of migration statistics more difficult largely because of the types of "migration" questions asked in those censuses. It is also important to remember that local area population statistics may be strongly influenced under de facto enumerations by the time of year, or even time of day, that is set as the official census time.

While de jure counts of population overcome a number of the types of problems discussed above, they also usually present a number of potential constraints. Most of these relate to the definition of "usual residence" adopted and the extent to which this definition is understood and accepted by the populace. That is, in addition to making special provisions for certain classes of individuals (such as persons in the military and students attending

schools or colleges that are far enough away from their parental home that they maintain a separate living quarters or are billeted in a group quarters), de jure enumerations require a very explicit set of criteria, spelling out who should and should not be enumerated as part of a given household. Even when these criteria are clearly spelled out, many individuals ignore them for certain longer-standing and more entrenched cultural criteria or for political reasons.

In a number of countries, for example, persons from particular ethnic areas prefer to be counted as part of their home area rather than as part of the area where they may actually reside. In other situations, this reason may combine with the custom of treating any family member who contributes to the maintenance of a household as part of that household no matter where that person may spend most of his time. Thus, a family member may spend 26 to 28 days out of a month living and working in a city and return to their "home" for only two to four days a month; yet they will view their usual residence as "home."

Problems such as these tend to result in under-enumerations for urban places and over-enumerations for rural areas. And, these problems are some of the primary reasons why a number of countries prefer to use a de facto basis for their enumerations. Again, it is important to note that, while the type of count conducted will not affect national totals, the extent of coverage at the national level cannot be assumed to be equal at the sub-national level. In this same sense, it is particularly important to bear in mind that differences in the extent of coverage between two censuses can have important consequences for the measurement of demographic change. If the coverage for an area improves over time (from one census to the next), or if the enumeration is more complete for later censuses, what might be assumed to be growth may actually be only a result of the improved enumeration.

A second set of basic parameters which impose constraints on the use of census data to assess the demographic consequences of development programme inputs relates to census geography. The most obvious constraining factor in this sense is the extent to which census geography coincides with development programme geography. In most countries of the world, census data are available for major political subdivisions (such as provinces or regions), as well as for at least one lower level of political unit (such as districts or states) within the major political subdivisions. The amount of geographic detail available below this level varies widely from country to country. But, in the vast majority of countries, separate statistics are usually available and tabulated for cities and/or towns above a certain specified size or functional status.

In addition to reporting or making available statistics for areas which are defined solely on a political/administrative type of criteria, many censuses also include some geography which is expected to reflect a functional criteria. 9/ The most common areal delimitations of this type relate to either very large urban agglomerations or to rural territory as distinguished from urban areas.

In reality, census geography of the types noted above will be useful for assessing the demographic consequences of development programmes when the programmes can be established as targeted for the specifically delimited areas. The major problems in this sense are that the boundaries for political areas are not always clearly established at any one point in time and/or that they frequently change from one census to the next.

In most instances, it is safe to assume that higher-level political demarcations will be more stable between censuses than lower-level political boundaries. However, it is also generally true that lower-level political demarcations are usually less clearly specified than higher-level political boundaries at any one point in time. What this, of course, means is that a great deal of caution must be exercised when comparing areas or places that may bear the same name at two different points in time.

Moreover, political boundaries tend in many countries of the world to be quite arbitrary and not to follow many of the physical and natural boundaries that might influence the planners' decisions on the target area for a development project. The types of functional areas usually demarcated for censuses do not usually help to overcome these problems. For the demarcations of larger urban agglomerations are usually either over- or under- bounded in the sense that their boundaries either exclude a significant portion of the population of interest or extend well beyond the population of interest and include peripheral areas which might be better excluded. The advantage of having over-bounded areas is that their boundaries are more likely to remain stable between censuses.

The situation when dealing with rural areas is even more constraining. Rural territory (and its population), no matter the particular definition of urban/rural employed in a country, is always determined in a residual manner. Also, quite frequently, the only boundaries given to it are those which distinguish it from urban territory and its population. That is, statistical definitions along this particular dimension portray "rural" merely as what remains after the urban is excluded. In many countries, or in their higher-level political demarcations, this results in a situation where the rural population is spread over a rather vast territory that has no demarcation other than those which separate it from the urban. If specific development programme inputs are not targeted at the entire rural territory and its population, it is difficult to identify the target and assess its demographic consequences.

Most censuses, however, contain far more detailed geographic information than what appears in published reports. Quite frequently, the geographic detail on census coding schemes goes as low as the enumeration area level. Although one constraint on this type of area data is that separate areas will not cross political or administrative boundaries, it is possible to aggregate areas to form a unit which would have boundaries not available for published statistics and which might demarcate a non-political/administrative area.

While the possibility of using data for small area demarcations such as these should always be thoroughly explored, it is equally important to note that these types of areas may be redefined from one census to the next. Another constraint on the use of such small area data is that it is not unusual to find that data on some key variables get suppressed in order to maintain confidentiality.

Furthermore, an additional source of unpublished data frequently available from censuses is sample tapes. These tapes provide a sample of individual census records, and afford the researcher the opportunity to obtain custom tabulations for any items contained on the census record. While these types of tapes tremendously enhance analytical possibilities in terms of the number and variety of tabulations that are possible, their use is, in general, seriously constrained by sample sizes and by the fact that geographic codes are omitted when the number of cases for a given place fall below a minimum level.

The final set of basic parameters we need to consider relate to time, the fact that in most countries of the world censuses are conducted on only a decennial basis, and that a number of the items asked in censuses have a time referent which is not the same as the census date.

The first of these factors is a particularly important constraint because it tremendously restricts not only the number of points in time when observations can be made, but also rigidly sets the intervals between observations. The resulting problems are many in this sense. Perhaps the most important of these is that the longer the interval between observations the more difficult it is to isolate the effects of development programme inputs from the effects of other factors. This situation is made even more difficult by the fact that we know remarkably little about how long it should take various kinds of programme inputs to have an effect on demographic factors, or, as suggested in our diagram above, how those effects should operate.

And researchers need to bear in mind that a number of key variables, measured through census questions, have a time frame which is different from the time point reference of the census. Some of these items will be dealt with in greater detail below. But, for now, it is sufficient to note that this time frame can vary from as little as a week or two prior to the census date right on up to the lifetime of persons involved. And, it should also be noted that the types of items usually handled in this manner include labour force items, income items, nuptiality and marital status items, as well as questions related to mortality, fertility and migration.

Although censuses have been used as a point of departure, it needs to be emphasized that the points made and/or the issues raised under each of the parameters of data apply equally to aggregate data no matter what its source. Indeed, if anything, the researcher needs to proceed with even greater caution when census data are used in combination with data from other sources and/or when reliance must be placed exclusively on data from non-census sources. That is, for example, when census data are used in conjunction with data from some

other source; it is important to know the extent to which the basis of the count from which the data are derived is the same, the degree to which the populations covered are demarcated in the same manner, and how frequently they represent the same point in time.

Similarly, there are a large number of non-population based variables which might be useful in assessing demographic consequences, the data for which might come from a variety of different sources and have been collected and maintained in different manners. Examples here might include data on some types of infrastructures where responsibility for collecting the data has been decentralized to a lower level authority, or certain types of services where the service providers may be government supported in some areas and privately supported in other areas. In instances such as these, there may (or may not) be standard procedures for collecting data. And equally important, if there are, then these may be considerably more strictly adhered to in some areas than in others.

Measurement constraints in the aggregate analysis of population change

In the previous section we examined some of the basic parameters which determine the structure of data systems and place constraints on the uses to which they can be put. In the present section, we turn our attention to a consideration of the internal quality of data systems and ask what is necessary to attain meaningful and accurate measurement from the data? In answering this question, it will help to refer back to the diagrams presented at the beginning of this paper and to the aggregate concept of population discussed. In terms of the diagram, we will first specify more clearly the conceptual meaning of demographic consequences (the dependent variable in the analyses we want to perform) and the requirements of data needed to measure these. Following this, we will discuss the measurement of some of the types of variables in the left-hand portion of the diagram.

The two criteria through which the quality of measurement is usually assessed are reliability and validity. Reliability is the consistency of values of a variable that is received with repeated measurements of the same variable (i.e., the extent to which a variable correlates with itself). Reliability is the absence of measurement error. Validity, on the other hand, is the extent to which a particular measure represents the theoretical concept it is intended to measure. Thus, while reliability issues are concerned with the extent to which we can measure a factor consistently, validity issues are centred around whether we are measuring the thing we actually want to measure.

From a logical perspective, reliability is a necessary condition for validity, while validity is a sufficient condition for reliability. That is, one cannot establish that a measure is measuring what it is that one wants to measure unless one can establish the reliability of the measure. But, even after one establishes that a measure is reliable, it still may not be measuring what it is that you want it to measure.

Luckily, in the case of measuring demographic consequences we can rather clearly demarcate what it is that we want to measure. And there are even fairly standard techniques for measuring most demographic phenomena. The fact that we are interested in consequences implies that we are interested in change which, in turn, means that we are interested in being able to measure phenomena at more than one point in time. Issues of reliability and validity are particularly important because in the measurement of change the potential sources of error are increased. 10/

Demographic consequences of development programme inputs can be thought of as involving three substantive types of measurement. In many instances, researchers will be interested in assessing the demographic consequences of development programmes in terms of the former's influence on the rates at which demographic processes are occurring. More specifically, the researcher will want to know if the rate of a particular demographic process is occurring at a slower or faster pace than it was prior to the programme input.

Alternatively, in other instances, the researcher may be interested in a more general change, such as how overall growth has been influenced by a development project, including the structure of growth in terms of the contributions of the components of change to the growth. Finally, demographic consequences can be thought of in terms of changes in the composition of populations. Compositional changes are always the result of demographic processes occurring differentially to age or age-sex groups. But, on occasion, one may be interested in those changes independent of the processes accounting for them.

In recent years, a great deal of attention has been given to the development of techniques for making demographic estimates from incomplete data. Those techniques are particularly important when working at the sub-national level because it is here that the direct data needed to compute many demographic measures are most likely to be missing or incomplete. The important issues of working with those techniques are dealt with in another paper. So, here I will focus only on the types of measures usually developed from direct data.

The most common measures of two (fertility and mortality) demographic processes are basic rates which require the use of data on the number of events and some denominator. While there is a great deal of theoretical and conventional agreement about what constitutes the occurrence of each of these events at an individual level, there is far less agreement about what denominators are most appropriate to use in different situations involving aggregate analyses of those processes.

And, frequently, some ambiguity concerning the meaning of the events also exists. In this sense, it is important to bear in mind that a third demographic process can have the same effect as each of the demographic processes mentioned above. That is, migration can result in the aggregate either gaining or losing in population.

The major difference between either migration or fertility or mortality in the aggregate sense is that fertility is only a means through which new members enter a population (and all do this at a specified age). Mortality is only a means through which members leave a population (in significant numbers at predictable ages). However, migration can serve each of those functions.

The point of theoretical or programmatic significance here is that, from the aggregate perspective, it is entrances and exits from the population and how they occur, that are important to understand rather than the risk of having an event occur to an individual in the population.

Similarly, not all subgroups in the population are equally likely to have demographic events occur to them. Thus, controls for age and sex are particularly useful, even in aggregate analysis, because those factors are highly correlated with the occurrence of all demographic events. From the aggregate perspective, however, the introduction of controls such as these are not intended to improve the measurement of risk at the individual level, but rather, they are intended to control the conditions under which the flows (entrances and exits) influence the aggregate and cause changes in them to occur. From the perspective of comparing several aggregates, or of comparing the same aggregate at more than one point in time, such controls create a demographic or structural equivalence.

The important point from the perspective of aggregate analyses is that we want valid measures of aggregate phenomena. And we must clearly understand and interpret our measures from an aggregate perspective. With this point in mind, we can identify a number of standard measures of demographic processes that are useful for assessing aggregate demographic consequences of development programmes.

Direct data on fertility

If data on the number of births in an area are available, there are a number of standard fertility measures that can be calculated. The most common of these is the crude birth rate which is merely the number of births divided by an estimate of the number of people in the population multiplied by a constant. Measurement error in this case results from the under- (or over-) registration of the events and from under- (or over-) enumeration of the population. Many of the factors contributing to measurement error along these dimensions have already been discussed in the previous section.

From a validity point of view, it is worth noting that the crude birth rate is a very legitimate measure of the number of new entrants into a population. The measure is unadjusted for age and sex composition; but, if the main concern is with the number of entrants into a population (through fertility) relative to the total size of the population, the crude birth rate is a good and valid measure.

All other measures of the fertility process involving the use of data on the number of events and counts of the population require that some data on the number of events should be available by age of mother and/or some data on the age-sex composition of the population. Although seldom used in

traditional fertility analysis, age and/or age-sex standardized birth rates could be particularly useful in the aggregate context of assessing the consequences of programme inputs.

Direct methods of standardization would require data on the age-specific birth rates of the population being studied as well as data on the number of persons (or females) by age in the standard population and the total size of the standard. When age specific birth rates are not available for the population under scrutiny, one can employ indirect methods of standardization which require data on the total number of births in the population being studied and its age composition, as well as a set of age-specific birth rates for the standardized population.

If standardized crude rates are compared over time with unadjusted crude rates, their basic interpretation in terms of entrants into a population remains the same as discussed above. But there is the added advantage that the effects of changes in age and sex composition can be taken into account, and, through the comparison, one can also disaggregate the change into that resulting from compositional changes and that resulting from factors other than compositional change. This latter by-product may be a particular advantage when the researcher has reason to believe that a programme input has both a direct effect on fertility and an indirect effect through a direct effect on age-sex composition.

Other more commonly used age-restricted measures of fertility, involving direct data on the number of births, include the general fertility rate, age-specific fertility rates and a number of measures derived from the latter, which include the total fertility rate and the gross and net reproduction rate. All of these rates have the same basic sources of measurement error as the crude birth rate plus the error resulting from age misreporting. In addition to this, the general fertility rate is frequently calculated from data on all births, but includes only women 15-44 in the denominator. And, as a result, this creates an upward bias to the extent that births occur to women outside of this age range.

In terms of the validity of the measures, all of the summary rates in an aggregate sense represent efforts to measure fertility independent of, or removing, the influence of demographic structure. Conceptually, the total fertility rate is perhaps easiest to interpret; and it has the added advantage that it can be calculated using procedures similar to those discussed above for indirectly standardizing the crude birth rate when age detail for births is not available.

While summary measures of the types discussed above are appropriate and useful measures of the relative number of entrants into a population through fertility, there are times when the researcher may want to know or measure other dimensions of the process than simply this. For many of these purposes, the most useful measures to be derived from direct data are age-specific fertility schedules. Such schedules ordinarily show the number of births in a given period of time by age of mother (in five-year intervals). These schedules can be strongly influenced by age misreporting for mothers on both the source for the numerator and the denominator.

While there are a host of measures that may be calculated from age-specific data, the most useful purpose to which such data can be put in the aggregate context is probably the examination of the consequences of programme inputs at specific ages. That is, under many types of development programmes, there may very well be reasons to believe that their consequences on fertility may not be uniform throughout the age structure; and we may expect their effect to be concentrated in certain specific ages.

In many situations, however, the researcher may be confronted with the problem of having no direct data from an independent source on the number of births. When this is the case, it is sometimes possible to obtain various measures of aggregate fertility from census or survey data. If the only data available are on the age composition of the population, it is possible to calculate a child-woman ratio (usually defined as the ratio of children 0-4 to women 15-(44 or 49) times a constant).

While the data requirements for this simple measure are minimal, it suffers from serious problems of reliability because it is directly affected by the under-enumeration of young children and by reported age misstatements for young children; and, from a validity perspective, the measure is also confounded by the fact that its value is influenced not merely by the level of fertility in a population, but also by its level of infant and early childhood mortality.

Some researchers have argued that the child-woman ratio can be improved upon as a measure of fertility when it is possible to link women directly to their own young children. This is frequently possible because data on censuses and in many surveys are collected on a household basis. One of the principal advantages of this procedure is that it becomes possible to compute age-specific ratios. However, these measures suffer from the same reliability and validity problems mentioned above. Also, the fact that children not living with their mothers may be systematically excluded should also be considered.

While each of the ratios discussed above is basically derived from items usually included in a census or general survey, the most commonly used measures of fertility from direct questions on the subject relate to "children ever born" items. In employing these items to measure fertility, it is important to know which women have been asked the question. In some cases it is asked of every married women and in others it is asked of all women.

Measurement error from these types of questions can result in part from women who are excluded, as well as from the failure to report some births, or from the over-reporting of births. Under-reporting is likely to increase at older ages and may be influenced by infant and early childhood mortality. Similarly, women who die prior to a census cannot report on their children born.

One of the major restrictions on the use of these types of data for assessing the consequences of development programme inputs is that they provide extremely limited information on current levels of fertility. That

is, since the questions elicit responses for lifetime or cumulative fertility, we can ordinarily tabulate the number of children women of different ages have had, but, we know nothing about when the children were born.

Nevertheless, when these types of data are available for successive time periods, it is possible to learn something about the fertility of segmented cohorts. And, one can treat the age distribution of mother's births as a synthetic cohort. In these types of instances, the resulting measures of "fertility" are affected not merely by the factors mentioned above as influencing reporting errors but also by age misreporting for the mothers and differential coverage errors between the censuses or surveys.

On the other hand, one of the principle advantages of "children ever born" data is that it is usually possible to tabulate it by the full range of characteristics collected for women in the census or survey. This, in turn, makes it possible to calculate a number of measures specific for these characteristics from this type of data. Fertility histories (or birth rosters) can also be collected from women and applied to measure fertility. These types of data can be applied in much the same manner as "children ever born" data, and have many of the same measurement problems and advantages. But this can add the dimension of spacing to the analysis.

Data on migration

From the aggregate perspective, migration represents another component of population change. As noted above, the basic difference between it and the other components of change is that its effects can be either positive or negative. That is, it can either add to the population or reduce the population.

Of perhaps equal significance is the fact that migration can also have the most dramatic effect on population composition and structure over a short period of time. Ideally, we would like to know not only the aggregate effect of migration in terms of the amount of total change which can be attributable to it, but also how this change came about in terms of the gross flows contributing to it.

It is also important to bear in mind the distinction between migrations (the process) and migrants (the persons who engage in the process). In the vast majority of instances, it is the latter that we have data on and that we employ to measure the former. However, from the perspective of aggregate population change, it is also most frequently not the migrations or migrants that we are most interested in but rather what might be better referred to as the effect of migration.

As such, it is the net effects of migrations that contribute to changes in the size of populations; and, it is the difference in the composition and structure of migration streams (in versus out) that contributes to changes in the structure and composition of populations. Taking these points into consideration, there are a number of specific types of data that must be considered in an effort to estimate "migration."

Estimates of the net effect of migration can be derived via residual techniques. These techniques are related to the basic demographic equation presented at the beginning of this paper. The major advantage of these techniques is that they require no specific data on migration. Thus, they can be used for areas (and populations) that can be clearly demarcated at two points in time and for which no direct data on migration exist. Conceptually, the major disadvantage of such techniques is that they will provide no information on the gross flows that have contributed to the net migration; and, in the vast majority of circumstances, there will be severe limits placed on the number and type of subgroups for which even net estimates can be made.

The vital statistics method is conceptually the easiest of the residual techniques to understand. It requires data on the counts of population for the area at two points in time, as well as estimates of the number of births and deaths in the area between the same two points. The difference between the counts of population are taken to represent the total change in the area. And the difference between births and deaths constitutes the amount of the change due to these processes. The degree of distinction between these two differences, or the residual of the total change after taking account of the change due to fertility and mortality, is assumed to be the net effect of migration. Furthermore, this method can be extended to produce estimates for any subgroup (relative to age, sex or socio-economic variables) restricted only by the availability of data.

Measurement error in this case will result from both coverage errors in the sources of the counts, as well as from the under- or over-registration (estimation) of vital events. The basis of the counts (de facto versus de jure) and the basis of the registration of events can also create measurement problems; particularly when the counts are derived from a de facto procedure. That is, it becomes difficult to know if differences in the counts are a product of differences in the numbers of short-term visitors or transients or differences in the number of residents. Similarly, any boundary changes occurring between the times of the population counts will result in "population transfers", which this technique will automatically attribute to migration.

A somewhat similar variant of the vital statistics method is the national growth rate method, or rather, as it is sometimes referred to, the United Nations method. This method is employed specifically to obtain estimates of net migration for sub-areas of nations. It requires only data on counts of the population at the beginning and end of an interval, and an estimate of the national growth rate.

The national growth rate is applied to each sub-national beginning of the interval population to obtain an estimate of what the population in the area would be at the end of the interval if it grew at the same rate as the nation. Then, the difference between the estimated and the enumerated population is treated as net migration.

Estimates of net migration arrived at through this procedure are extremely crude. The major problem with this technique is that the researcher is left with no alternative other than to assume that the rate of population change

from the excess of births over deaths in each of the areas for which the migration is being estimated is the same as for the entire country. That is, one assumes that there are no geographically based differentials in fertility and mortality rates, and also that all of the growth differentials between areas are solely a result of migration.

Survival rate techniques of estimating net migration are probably the most efficient of all the residual techniques. Their major advantages are that they require no vital statistics, they yield age-sex specific estimates of net migration, and a whole host of adjustment procedures have been developed to be utilized in conjunction with them to help to reduce errors. Essentially, the technique requires only age-sex distributions for the areas for which migration is to be estimated at two points in time, together with sets of survival ratios depicting the age-specific schedule of mortality in those populations. The age-specific survival ratios are then applied to the initial population to produce an expected number of survivors in the age group over the time interval. The difference between this expected number of survivors and those actually enumerated in the age group at the end of the interval is assumed to be the product of net migration at each specific age. Summing these differences over all ages yields an estimate of the total net migration in the population.

The major sources of measurement error associated with estimates made from these procedures derive from coverage errors in the enumerations, from misstatements in the reporting of age and from the failure of survival ratios to accurately depict the level, timing and age pattern of mortality. Survival ratios are generally calculated from either the stationary population of a life-table or from national census data. There are three alternative techniques (forward, reverse and average) for applying these. In the following discussion of the assumptions and sources of error in the survival rate methodology, the focus will be primarily on the forward technique, because it is by far the most frequently employed.

With respect to the handling of deaths among migrants, the forward technique (because the survival ratios are applied to the population at the beginning of the interval) implicitly assumes that all migrants who died during the migration interval did so at the beginning of the interval. If the reverse technique is used, in effect the opposite assumption is made about deaths. That is, because the "revival" ratios are applied to the population at the end of the interval, it is assumed that deaths occurred to in-migrants before they actually migrated into the area. No allowance is made for deaths to out-migrants from the initial population.

Thus, the forward survival ratio technique substitutes deaths to out-migrants for deaths to in-migrants. Whereas, the reverse technique delegates deaths to in-migrants for deaths to out-migrants. It is because of this problem in the handling of deaths that some persons have suggested employing both techniques and "averaging" them to gain a truer estimate of the net migration. Whether this averaging will significantly affect the error resulting from the handling of deaths will, to a large degree, depend upon the extent to which one can assume that there are significant mortality differentials between the in- and out-migrants from an area.

The two remaining sources of measurement error associated with these procedures relate specifically to the survival ratios employed. The first of these is the extent to which the population from which the survival ratios are calculated is a closed unit, affected only by natural increase. If survival ratios are obtained for stationary populations from life tables, this is not a problem. But when one employs survival ratios calculated from a census, closure is difficult to attain.

In most instances, the best that one can come to in simulating closure from censuses is to employ national data for the native population. But, in reality, these figures are always affected by international migration and various practices which influence the coding of residence for special groups and persons living abroad.

A second source of error directly related to the survival ratios is the extent to which they actually simulate the mortality schedule in the population for which estimates of net migration are to be made. If the data are available, it is possible to construct life tables specific for the area under study. Then, from the perspective of attaining as accurate a representation of mortality as possible, this would represent an ideal solution.

Census survival ratios, however, have a built-in correction for under-enumeration and misstatement of age, while life table survival ratios do not. So, this advantage is sacrificed when life table survival ratios are used. That is, survival ratios calculated from census data adjust themselves downward if the population is over-enumerated at time one, and adjust themselves upward if the population is under-enumerated at time one. Similarly, the reverse situations apply if the under- or over-enumeration is at time two.

The stationary population of the life table does not contain these self corrections, since it has been smoothed and is only a theoretical population. This solution to the mortality problem means sacrificing the corrections achieved through the use of census populations to construct the survival ratios.

A better solution, which preserves the advantages of both types of survival ratios, involves using the life table survival ratios to adjust the census survival ratios for local mortality conditions. To employ this procedure, three sets of survival ratios are needed: one for the area in which the estimate of net migration is to be made, another for the assumed closed population and a set of census survival ratios for the latter. At each age, the difference between the two life table survival ratios is added or subtracted (depending on the sign of the difference) from the census survival ratio. Even in this case, however, it is important to bear in mind the possibility that the amount and extent of under- or over-enumeration in any particular sub-area of a country may be different from that which is reflected in the self-corrections obtained for the survival ratios from national level census data.

In addition to having migration data made available through residual techniques such as those discussed above, many censuses and an increasing number of surveys also contain questions which allow us to obtain direct data on migrants. The most commonly included item of this type asks for place of birth. And the most common uses of this type of item derive from its cross-classification with other items. Indeed, in addition to the fact that this type of information can be used to obtain estimates of gross flows of migration and information on migration streams, one of its major advantages is that the limits to the number of items with which it can be cross-classified are set only by the items included in the census or survey instrument.

The simplest and most direct cross-classification of this item for the purpose of obtaining migration estimates is with place of current residence. When place designations are exhaustive of all subdivisions of a country, it is possible to obtain a matrix which shows the number of persons resident in all areas by their place of birth; and this, in turn, can be summed for rows and columns to obtain estimates of the lifetime movement to and from each area to all other areas.

One major problem with migration estimates derived in this way is that they have no time referent for the migration, nor do they provide any information on migrations per se. Persons who moved and died are automatically excluded, since they were not alive at the time of enumeration, just as persons who return to their place of birth are excluded from the migrant category. What information does exist is on persons who are living in a place other than where they were born at the time of the enumeration; and even this is usually restricted to the native population.

When these types of data are available for two consecutive censuses, they can be used to estimate the intercensual migration to and from each area identified as a place. The estimates of intercensual migration are merely obtained by taking the differences in the number of persons moving into and out of each area at each point in time. Estimates of migration derived in this manner, however, are biased by deaths to migrants and by return migration to places of birth, and even by migrations which involve movement from one place of non-birth to another place of non-birth. As a result, there is a general consensus among students of migration that these types of estimates produce excessively low estimates of migration.

Finally, it is worth noting that for a number of countries there is a relatively high non-response rate to questions on place of birth. This results from the fact that in some countries some people simply do not know the place of their birth, while others identify places that cannot be coded because they no longer exist. Similarly, boundary changes can result in some persons identifying their place of birth incorrectly; and, in still other circumstances, a properly identified place of birth may not have the same connotation for all persons.

This situation arises frequently in countries where people may go to a place to give birth to a child other than where they and the child will normally reside; while others give birth to their children in their place of normal residence. A last caution in using these data is that, when they are included in censuses, they are frequently included in the sample portion of the census and thus also contain sampling error.

Other types of questions which solicit direct information used to estimate migration include questions on place of residence at some earlier fixed date, duration of current residence and/or last previous residence. The data produced by the last two categories here are most useful when used in combination with one another, although some national censuses still ask one type of question, but not the other.

Duration-of-residence type questions yield data that can be useful for studying the in-migration to specific areas; but, it is important to know the areal detail referent used in the question and to remember that any estimates of migration will be biased by deaths to migrants who died prior to the census, as well as by migrants into an area who left the area prior to the census. When this type of information is combined with data on place of last residence, it is possible to reconstruct streams of migration to and from specific areas for specified periods of time. The same type of thing can be done with data on place of residence at some earlier fixed point in time. But, the difference is that with the latter one has no control over the period to be observed.

As noted above, one of the primary advantages to using data from these types of questions is that it is possible to cross-classify this information to obtain additional data on the characteristics of migrants and non-migrants, even to the point of origin and destination. While the information on characteristics obtained in this manner can be useful for some purposes, it is important to bear in mind that it is information on only one point in time, and also, after the migration occurred.

Up to this point, we have focused almost exclusively on the sources of data for estimates of migration and some of the major sources of error associated with each of these. In some instances, where the concern is with assessing the demographic consequences of development programmes, information on the absolute number of migrants (net, in and/or out) or on the number of migrants by some characteristic may be useful. In most instances, however, the researcher will want to express the volume, composition and structure of migration in some relative term. Such is the case when working from the aggregate perspective; and this is so particularly when concern is with the consequences of programmes affecting a specific population. For then, in most instances, we would like to employ that population as the basis of relative measurement.

Accordingly, in many situations where the consequences to migration of development programmes is being measured, the migration (be it net, in or out) must be related to the population in the area where the programme has been initiated. In this type of circumstance, the interpretation of an out-migration rate is not really different from the interpretation of a mortality rate. And the interpretation of an in-migration rate does not differ from that of a fertility rate. Likewise, a net migration rate is in most respects synonymous with a rate of natural increase.

This is not to say that there are never times when an alternative population base might not be employed (such as the population in areas where in-migrants come from). But, in such cases, the resulting measurement is difficult to interpret from the aggregate perspective of the receiving population.

Another important question relating to the validity of migration measures and the influence which denominators have on it centres around time. Annual data on migration will be available only in rare circumstances. In most circumstances, the period of observation will be either 10 years (an intercensual period) or five years.

This has traditionally posed a problem among researchers interested in measuring the results of migration because areas substantially affected by migration (net, in or out) have their rates biased by the process. That is, if a population has heavy out-migration and an end-of-the-period population is used as the denominator, the resulting risk will be overstated. While, if a population has heavy in-migration and a beginning-of-the-period count of the population is used as the denominator, the risk will be understated.

Largely because of this, it is usually recommended that a mid-period estimate of the population should be employed as the denominator, even though the interpretation of net- and in- rates of migration at a micro-level is difficult when a sub-national population is being analysed. From an aggregate perspective, it makes better sense conceptually to employ as the denominator a population count that represents the population at the beginning of the migration period.

If data are available for only one period, or if only one period is to be observed, rates calculated using this base can easily be interpreted as indicators of the role of migration on the aggregate over the period. When multiple rates are to be compared for several different periods, and when these periods span large swings in the volume of (and direction of) migration, the differences in the resulting rates will be influenced by the effect the migration has on the observed population in a similar manner, as discussed above. In such instances a mid-period denominator is again preferable.

Rates of net migration only have interpretative meaning at the aggregate level. They represent the effect of migration on population change. And, as such, they are usually expressed as percentages or proportions of change, using the population at the beginning of the period as a base. A number of researchers have noted that there is no such thing as a "net migrant" in the real world. However, while this is true and a problem at the micro level, it is not at the aggregate level.

What must be appreciated is that rates of net migration are no different from rates of natural increase in the sense that they indicate a direction and an excess; but they tell us nothing about the volume of entrants and departees. In those instances where the concern is with measuring the effect of programme inputs on migration's contribution to population change, rates of net migration are useful.

In many instances, it is necessary to know more than the total effect of the process; that is, how the components of the net rate have changed. From the aggregate perspective of population change, it only makes sense conceptually to calculate both in- and out-migration rates, using the population of interest as the denominator. In these cases the rates of migration are interpretable in the same manner as fertility and mortality rates. They reflect the entrances and exits from the population.

Summary

The measurement of demographic consequences at the aggregate level largely derives from an in-flow/out-flow concept of population. Most standard demographic rates can be readily interpreted from this perspective, and are therefore useful for aggregate analysis. Major problems, however, arise when these measures are needed at the sub-national level and/or when they are needed for specified periods of time. These problems basically derive from the structure of data systems with respect to the time at which data are collected, the demarcation of areal units and whether the basis of the population count reflects the de facto or de jure population of the area.

The reliability of direct data used for many standard demographic measures has been discussed in some detail. A number of sources of error for each has been identified, and discussions of fertility and migration have described measures that could be obtained directly from census data as well as those requiring an independent source for the event data; providing the important point, in this sense, that not only in many countries is data availability likely to determine which measures can in fact be used, but also that many measures considered deficient at the micro-level have clearer meaning at the macro-level.

Notes

1/ A basic review of the development of population theory can be found in The Determinants and Consequences of Population Trends, vol. 1 (United Nations publication, Sales No. E.71.XIII.5). See also Gordon E. De Jong and Robert W. Gardner, eds., Migration Decision Making (New York, Pergamon Press, 1981) for a contrast of aggregate and individual approaches to the study of migration.

2/ David Sly, "Migration and the ecological complex," American Sociological Review, vol. 37, No. 5 (1972), pp. 615-628.

3/ The Concept of a Stable Population: Application to the Study of Populations of Countries With Incomplete Demographic Statistics (United Nations publication, Sales No. E.65.XIII.3).

4/ Tom Burch, "The structure of demographic action", Journal of Population, vol. 21, No. 4 (1979), pp. 279-293; and Burch, ed., Demographic Behaviour (Boulder, Colorado, Westview Press, 1980).

5/ W. S. Robinson, "Ecological correlations and the behaviour of individuals", American Sociological Review, vol. XV (June 1950), pp. 351-357.

6/ David Sly and M. Wrigley, "Migration decision making and migration behaviour in rural Kenya", Population and Environment, vol. 8 (1986), Nos. 1 and 2, pp. 48-62.

7/ Albert I. Hermalin, "Spatial analysis of family planning program effects in Taiwan, 1966-1972", Papers of the East-West Population Institute, No. 48 (April 1978).

8/ A complete listing of census items and procedures can be reviewed in Principles and Recommendations for Population and Housing Censuses (United Nations publication, Sales No. E.80.XVII.8).

9/ Various alternatives of this type and their advantages and disadvantages are discussed in S. Goldstein and D. Sly, eds., Basic Data Needed for the Study of Urbanization (Dolhain, Belgium, Ordina Editions, 1975).

10/ Consequences are frequently assessed, or at least implied, from research designs which use only measurements at one point in time. The statements made here assume that consequences are measured changes in a variable.

C. Indirect demographic estimation of subnational populations

Kenneth Hill*

The direct demographic impact of most types of development project will be limited to the population of a particular area, that is, to a sub-national population. Both the area and the intensity of the demographic impact, however, will depend on both general and specific characteristics of the development project. Some types of projects are likely to affect the population of a fairly clearly defined area, affecting not only those directly involved with the project but also others living in the area. An example of this type is an irrigation project; the area served by irrigation is clearly defined, and economic, social and health effects of the project will be felt by most of the population of the area. For other types of projects the area affected may not be clearly defined (for example, a road construction project); or the impact on the people within the area may vary widely between those directly involved and the rest (for example, an industrial project). Thus, estimation for sub-national populations will usually be necessary, but rarely sufficient, for examining the demographic impact of major development projects. This paper, however, is restricted to consideration of the issues arising in demographic estimation for small areas, rather than considering of how such estimates can be incorporated into an evaluation of the demographic impact of major development projects.

The nature of the problem

Most developing countries lack the full battery of data sources (regular, recent and reliable census and vital registration data, supplemented by *ad hoc* surveys) that provide the conventional basis for demographic measurement. As a result, demographic measures in most developing countries are generally obtained in other ways, ranging from the evaluation and, if necessary, adjustment of conventional sources through the use of census or survey questions as surrogates for a vital registration system, to the establishment on a sample basis of an entire conventional data base. In all cases except the last, evaluation or transformation procedures are required to arrive at demographic estimates, but these procedures often assume that the population under study is closed to migration (they often assume also that the population age distribution is approximately stable, but deviations from this assumption introduce little distortion). Such an assumption is entirely unsuitable to our purpose, because a development project may be expected to stimulate migration, and indeed migration is likely to be one of the demographic effects to be studied. Further, for our current purpose, we are primarily interested

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in measuring trends and differentials in trends, especially the intervening period from before to after the time when the project became operational, whereas the procedures available tend at best to provide measures of an average trend over an extended period. Even the last case, the sample conventional data base, is unlikely to provide the required information, since the sampled population in the target area may not be large enough and since the information is likely to require evaluation. It is worth stressing, however, that small numbers are not necessarily a general problem; they will limit the use of certain sample data sets, but census and vital registration data should provide adequate numbers for most purposes. Data obtained from questions included in a built-in census sample may be an exception.

The nature of the problem thus requires that a suitable analytic strategy should have three major characteristics. First, data have to be used for specified small areas that can be identified as likely to be affected demographically by the project. If particular data sources cannot be disaggregated to the extent required, their value in the analysis will be greatly reduced. Second, migration has to be taken into account explicitly, not only in the methodology used for estimating fertility and mortality but also as a parameter of interest in its own right. Third, a series of observations over time is needed to improve the precision of trend estimates relative to those available from the internal detail of individual applications.

It will be clear to the reader that the appropriate analytic strategy for a particular case is determined by the data available. It would be impossible in this paper to attempt to define suitable strategies for all, or even a majority of, likely cases. Instead, some general approaches will be indicated that can be used if the necessary data at the required level of disaggregation are available, and some methods will be proposed for using available data that are particularly suited for application to sub-national populations. It is assumed throughout that though sub-national populations are the focus, adequate numbers of observations are available to make chance fluctuations negligible. Before starting on specific strategies, however, it will be useful to review the demographic parameters, indirect methods and data sources relevant for small area studies.

Catalogue of applicable indirect techniques

Fertility

Measures of fertility of most interest will be total fertility and indicators of pattern such as age - or marital duration - specific rates or cohort parity progression ratios.

(a) P/F ratios: These ratios compare cohort measures of fertility, average numbers of children ever born (P), with period measures, cumulated period-specific fertility rates (F) (Brass, 1964; United Nations, 1983). Under certain circumstances the ratios can be used to adjust the period-specific rates, usually calculated from registered births or births reported as occurring in a specified period before a survey, for coverage. However, the pattern of the ratios (by age or duration of marriage) provides

an internal indication of fertility change, and the cohort and period data can also be combined to synthesize cohort parity progression ratios (Brass, n.d.). Period-specific ratios can be calculated if data are available for two different times, with improved prospects for disentangling the effects of trends and errors;

(b) Own children: Children are matched to the most probable mother within each household; the child's age provides the period of the birth, and, in combination with the mother's age, the age of the mother at the time of the birth. The number of matched children of each age is inflated to allow for the corresponding number of unmatched children. Known or estimated mortality rates are used to allow for deaths of children and of women. Age-specific fertility rates (and total fertility) are then calculated for years or periods as far back as 15 years preceding the survey (Cho, 1973);

(c) C/B ratios: These ratios compare registered births adjusted for known or estimated mortality (B) with children of corresponding age recorded by a census (C), thus providing measures of birth registration completeness. Unless such completeness is changing, registered births provide the most sensitive indicators of trends in the level and pattern of fertility;

(d) Maternity histories: Maternity histories provide direct measures of fertility level and pattern over a lengthy period prior to the data collection, but they are included here for the sake of completeness, as being worth considering for inclusion in an ad hoc survey, and as possibly requiring evaluation vis-à-vis other sources.

Mortality

It is convenient to divide mortality into two parts, child mortality and post-child mortality, partly because the data and methods used are quite different and partly because project impacts may be expected to be different for these two age ranges.

Child mortality

Suitable summary measures include the probabilities of dying by age two, q(2), and age five, q(5); the infant mortality rate and child death rates are less suitable as summary measures. However, age patterns of child mortality are of importance; the probabilities of dying by age one, ${}_1q_0$, and between ages one and five, ${}_4q_1$, provide suitable measures.

(a) Children dead of those ever borne. The proportions dead of children ever borne by women in standard age or duration of marriage groups provide measures closely related to probabilities of dying between birth and exact ages of childhood. Adjustment for fertility pattern provides estimates of such probabilities of dying (Brass, 1964; Trussell, 1975) and of broad trends in child mortality over a period of some 15 years (Feeney, 1980; Coale and Trussell, 1978). The data required, also relevant to fertility analysis, can be collected by any retrospective survey.

(b) Maternity histories. If the date of birth and, if applicable, the date of death are recorded for all the children borne by surveyed women, child mortality rates can be calculated by age, period and cohort for some considerable period in the past, providing measures of age patterns and trends of infant and child mortality. In practice, small numbers typically limit disaggregation and necessitate grouping into five-year or similar periods. The measures are direct, though the sample universe is limited to children whose mothers are available for interview (Somoza, 1980);

(c) Survival of previous child. As part of a continuous monitoring system, information is obtained at first contact with a reference child (as close to its birth as possible) about the survival of the mother's previous child, if any. The proportion dead of such previous children closely approximates the probability of dying by age two, q(2), in the recent past (Brass and Macrae, 1984);

(d) Adjusted registered deaths. Although infant deaths are frequently under-registered, evaluation against results of methods a, b or c may show that registration of child deaths (say, at ages one to four) is complete enough to chart trends adequately.

Post child mortality

The most suitable summary measure for general purposes is life expectancy at age ten, e(10). However, for certain projects more limited age ranges such as that of the labour force or that of reproductive women may be of primary interest, in which case measures of temporary life expectancy between ages a and a+b, ${}_b e_a$, may be more suitable. Certain types of data error may also make the use of temporary life expectancies advisable.

(a) Evaluation and adjustment of registered deaths. A range of techniques has been developed for evaluating the coverage of death registration relative to population recording (Brass, 1975; Bennett and Horiuchi, 1981; Hill, 1986). The techniques all assume constant relative coverage by age, thus being mainly applicable to deaths after childhood, and a closed population; if only one population age distribution is available, stability also has to be assumed. The techniques can be applied to survey data on deaths by age during some specified time period. Suitably adjusted deaths by age provide valuable detail on age patterns of mortality, and if trends in coverage can be established, so too can trends in mortality.

(b) Intercensal survival methods. For a closed population, intercensal cohort survival and its age-specific growth rate analogue (Preston and Bennett, 1983) provide measures of post-childhood mortality. However, changes in census coverage can have a dramatic effect on the mortality estimates, as can systematic age misreporting, which is likely to have particularly serious effects on age patterns of mortality.

(c) Survival of close relatives. Procedures have been developed for estimating adult mortality from information on the survival of close relatives, notably mothers, fathers and first spouses (Brass and Hill, 1973;

Hill, 1977; United Nations, 1983). Proportions by age group with the specified relative surviving are converted through the use of models into life table survivorship probabilities to which approximate time locations (Brass and Bamgboye, 1981) can be assigned. However, the final measures are averages over a wide range of ages and periods, so neither age patterns nor trends of mortality are measured with precision.

Migration

Migration is the most problematic demographic parameter not only to estimate but also to decide on a suitable parameter to estimate. Unlike fertility and mortality, migration is reversible, the events themselves are ambiguous, depending on geographic definition, and at least for immigration there is no clear definition of the population at risk. For most purposes, the simplest measure, the crude net migration rate for the project area, is probably the most suitable. This rate can be defined as the balance of immigrants (persons changing their place of usual residence into the area) over out-migrants during a specified period divided by the person-years lived by usual residents of the area during the period. It is important, however, to recognize the shortcoming of this measure, particularly for comparison purposes, namely that the rate will depend on the initial population of the area. An irrigation scheme in an uninhabited desert will give rise to an infinite net migration rate if only one family moves. For some purposes absolute numbers will be more suitable. On the other hand, the crude net rate has the advantages that it will not depend greatly on the size of the area, since peripheral moves will largely cancel out, and that it will not be greatly affected by the initial population age distribution.

(a) Multi-round or continuous surveillance surveys. The only method of measuring migration that does not require prior information about mortality is continuous surveillance or a multi-round survey with not more than six months between rounds. The initial population of the area is enumerated, and all gains and losses through migration are subsequently recorded; with a short interval between rounds, losses of immigrants changing their residence or dying between rounds will be small. However, the cost of such a survey will be high and the use of sampling difficult since it would then be necessary to ascertain where gains had come from and losses gone to.

(b) Intercensal residuals. A conventional approach to the estimation of net intercensal migration is to project the initial census age distribution forward to a second census, using known or assumed survivorship ratios, and to interpret the difference between the projected and recorded population at the date of the second census as the survivors of net intercensal migration. However, the calculations are inconvenient if the intercensal interval is not a multiple of five years, and the result in terms of surviving migrants only is unsatisfactory. Recent results on the gains, losses and growth of populations by Preston and Coale (1982) and Coale (1985) avoid some of the methodological problems, but still require estimates of mortality.

(c) Analysis of birthplace data. A single set of birthplace data provides little more than an indication of the situation at a particular moment resulting from lifetime migration. However, two or more sets of such

data provide considerably more information. Intercensal residual methods can be applied to the population born in the project area, estimating primarily emigration between two observations, and to the population born elsewhere, estimating primarily immigration.

(d) Analysis of other migration data. Censuses and surveys sometimes collect additional data relating to migration, typically duration of residence or place of residence some specified period such as five years earlier. Although neither type of data lends itself particularly to the application of indirect estimation, both can provide useful bases for controlling for the effects of migration on other indirect estimation procedures. Duration of residence, for example, can be used to classify the population into recent migrants (less than five years residence), established migrants (five or more years) and non-migrants. Residence five years earlier provides similar classes, and as a census question permits comparisons between the residents five years earlier, of some other area who move into the project area and those who do not, a combination of quinquennial censuses and a question on residence five years earlier provides a basis for extensive checks on reporting accuracy and census coverage (see annex).

Pros and cons of different data sources

Four types of data source, each with its own strengths and weaknesses, are likely to contribute to demographic estimation for small areas. The types, with their strengths and weaknesses, are described below.

Population census

A population census approximates complete coverage, and can provide at least the age and sex distribution of the population of the project area without sampling error as long as the census results are tabulated for small enough areas. A series of censuses provides information on the evolution of the population of the project area provided that area definitions can be consistent over time. Additional information on birthplace, sufficiently disaggregated to permit the identification of the project area, greatly enhances the value of the data, particularly for a series of censuses. Other demographic information, such as children ever born, children surviving, recent births and recent deaths, provides a basis for standard estimation techniques, of particular value for a series of censuses and in combination with birthplace data. Censuses are thus of enormous potential value. The problems with them are that they are taken rather infrequently, at dates unlikely to be related to the project, and that it may often prove difficult or impossible to tabulate past censuses for a project area.

Vital registration

Registered births and deaths can, by virtue of their continuity, provide the most satisfactory basis for monitoring fertility and mortality trends. However, vital records will be of great value only if they are tabulated for places of usual residence (of the mothers or of the decedent) sufficiently small for the project area to be identified and if the completeness of registration can be evaluated with sufficient precision to allow valid

adjustment even when completeness may be changing. The first condition is unlikely to be met in many countries; but even when it is not met, registration data may be of some value, for instance for selecting a mortality pattern suitable for the project area on the basis of national adjusted deaths. The second condition requires the use of methods of evaluation that do not assume that the population is closed to migration or, more commonly, the application of closed population methods to sub-groups of the population that can be regarded as approximately closed.

National demographic or household sample surveys

Such surveys are unlikely to prove useful because the sample may not represent the population of the project area adequately. However, the data from such surveys may prove to be of some use, for example as a basis for choosing models for the analysis of project-area data from other sources.

Project specific surveys

A survey designed specifically to evaluate project impact can include whatever data are needed for the specific area of expected effect. A possible design would start with a baseline survey collecting information on age, sex, children ever born and surviving, birthplace and residence five years earlier and a full birth history; rounds every six months would record the occurrence of births, deaths and migrations; a final survey would repeat the baseline survey to provide a basis for validating the information from the multi-round system. Such a design would provide a remarkable data base, but would be prohibitively expensive to implement and would still not answer all questions of interest, such as how immigrants from a particular region might differ from non-migrants still in the region of origin. More typically, a survey should be seen as a way of supplementing existing data from other sources, and should be designed accordingly. Often a single-round retrospective survey incorporating the questions necessary for application of the methods above will be sufficient, in combination with a census a few years earlier, to provide before-and-after measures.

Analytical strategies

Most existing techniques for indirect estimation of demographic parameters assume the population be be closed to migration, whereas we are interested here in estimates for open populations. Two possible strategies suggest themselves for resolving this incompatibility. The first is to define a population that is essentially closed to migration but most or all of which is relevant; existing techniques can then be applied to this population normally. The second is to develop methods that do not assume a closed population; such methods can be applied to the whole population of interest. The first strategy can be implemented by regarding the population born in a particular area, wherever it is recorded, as a closed population (a good approximation in the absence of substantial emigration) or even, if out migration from the area is small, regarding the population born and recorded in the area as a closed population. The remaining population is an open population concentrating all observed migrants. The second strategy requires the development of new methods, something that cannot be expected to happen

overnight, though some suggestions are given below. The rest of this section discusses each of the methods outlined and suggests how either the first or the second strategy can be applied to their use with open populations. No examples will be given because both strategies require the use of data disaggregated in ways that are not readily encountered in published sources.

P/F ratios

The second strategy, of working with closed (born in the area) and open (born elsewhere) populations can be applied. If calculated from census or survey data on births in the preceding year, the denominators of these ratios will closely approximate current fertility in the area, for both the closed and open populations. If registration data are used, it will not normally be possible to distinguish births for the two populations, and the additional assumption is needed that age patterns of fertility are similar for the two populations. If emigration is low, or is not related to lifetime fertility at the time of migration, the average parities for the closed population will also correctly reflect lifetime fertility of those born in the area. Thus P/F ratios for the closed population can often be interpreted normally. If emigration is not low, the assumption that it has not been related to lifetime fertility can be checked, using census data, by comparing average parities for the women both born and recorded in the area with those of women born in the area but recorded elsewhere; if the two sets are similar, the assumption is supported. However, the assumption is not necessarily invalid even if the sets are different, since the differences may have arisen after migration. A further test can be made if data are available on residence five years earlier or duration of residence. In these circumstances the comparison should be with women born in the area, recorded elsewhere, but resident in the area five years earlier (or resident in the current place for less than five years). For the open population, the P's will reflect both fertility elsewhere prior to migrating and subsequent fertility in the area, and the P/F ratios will thus reflect data errors, fertility trends and fertility experience prior to moving. Such ratios will be difficult to interpret, though once again additional information on duration of residence (or residence five years earlier) will be useful, since ratios for young women born elsewhere, but resident in the area for at least 10 years (or less satisfactorily, five years earlier), will be little affected by pre-migration childbearing. Thus P/F ratios are likely to prove useful as long as the population of the area can be subdivided into those born in the area and those born elsewhere, particularly for the non-migrant population.

Own children

If emigration is slight, the own-children procedure can be applied to the population born in the area to estimate fertility of the closed population with only two complications. The first complication is that the mortality rates used should be those applicable to the closed population. Such mortality rates can often be estimated, however, and even when they cannot the effect of using inappropriate rates on the fertility estimates is not very large. The second complication is the non-own children; the children not allocated to a mother cannot be allocated to the closed or open population either, so non-own children factors cannot be calculated separately for the

two populations. Normal practice is to use the population non own factor for all sub-populations; any error thus introduced will be small as long as the factor is small. However, a specific non-own factor can be estimated if information is available on children ever born and surviving. For each woman, the number of non-own children is equal to the difference between children surviving and matched own children. The great majority of the children of women under age 30 or 35 will be under age 15, so the ratio of surviving to own children for these women can be used as the non-own factor, adjusted as necessary to reflect any strong age-pattern in the normal non-own factors. If emigration is substantial, but not related to fertility, own children fertility estimates can still be used for the area born population, though non-own factors should be calculated from the difference between children surviving and own children, since the estimates will otherwise be biased if children of emigrant mothers remain behind. As with P/F ratios, the assumption that fertility and emigration are not linked can be checked by comparing estimates for emigrants and non-emigrants, or if the data permit, recent emigrants and non-emigrants.

Own-children estimates for the open population encounter the same two problems of mortality measurement and obtaining non-own factors plus the additional one that the resulting estimates include fertility both in and out of the area. Children ever borne and children surviving data should provide suitable measures of mortality for the own children of these women, and non-own factors, calculated on the basis of the ratio of children surviving to own children, should avoid problems of children left with relatives in the area of origin. The third problem can be partially resolved, if the necessary data exist, by further disaggregating the open population by residence five years earlier (or duration of residence), and applying the own-children procedure to the female population resident in the area five years earlier (or resident for five or more years).

The own-children procedure can produce useful estimates of fertility for a given area as long as the census data can be sufficiently disaggregated by area of residence, area of residence five years earlier (or duration of residence) and place of birth. Only minor modifications of the standard methodology are needed. The estimates will, of course, suffer from the usual shortcomings of fertility estimates derived from age distributions, mainly distortions arising from age misreporting, but applications to a series of data can often provide a basis for distinguishing between trends and error patterns. The method is also rich in detail, providing levels and age patterns of fertility for up to 15 years before the census.

C/B ratios

These ratios can only be calculated if registered births by year can be tabulated for the project area. The relevant numerator is the population born in the area recorded anywhere in the country, assuming emigration is negligible. The denominator, registered births by year, has to be adjusted for mortality, multiplying by survival from birth to a given age $a,{}_1L_a/l(0)$. Strictly speaking, the mortality rates used for this forward survival should be those of all the children born in the area, though since mortality early in life represents a large proportion of all child mortality,

and since the cumulated probability of emigration at such early ages will be small, the mortality experience of the children of women born and resident in the area will provide an adequate approximation. As with the own-children method, the calculation of C/B ratios for overlapping time periods (using two census age distributions) will help to distinguish trends (in registration completeness) from errors (particularly of age reporting). However, even if C/B ratios are sufficiently consistent over time to provide usable adjustment factors for registered births, the calculation of fertility rates will still require the estimation of suitable denominators, the numbers of resident women of each age group in each year; and the rates will refer to the whole population of the area, rather than to its closed or open segments.

Children dead of those ever borne

If emigration is negligible, standard analysis of the proportions dead of children ever borne by women both born and resident in the area will provide valid estimates of child mortality levels and time trends for this group of children. Substantial emigration introduces three potential problems: first, that children who move out without their mothers may experience mortality risks very different from those of the area; second, that the children of women who move out may have experienced different mortality risks from those of women who do not move out; and third, that women who move out may have different lifetime fertility from those who remain, thus affecting the parity ratios used to adjust proportions dead for fertility pattern. The first problem is unlikely to be serious, since the highest mortality risks are experienced at early ages before the cumulated probability of having emigrated is likely to be large. The magnitude of the second problem can be examined by comparing proportions dead for women born and resident in the area with those for women born in the area but resident elsewhere. The power of this comparison will be increased if the women resident elsewhere can be divided into those who moved recently (the appropriate comparison group) and those who moved less recently, using data on residence five years earlier or duration of residence (the latter being less satisfactory since it assumes single moves only). The third problem could have a substantial effect if, for example, women with fewer than average children were more likely to move out, inflating average parities and deflating their ratios. As with the second problem, it may be possible to examine the magnitude of any bias by comparing average parities for area-born women resident and not resident in the area, particularly if the latter group can be divided into recent and not recent movers.

Proportions of children dead for women of the open population will reflect child mortality both in the area and elsewhere, and indeed predominantly the latter for children born elsewhere because of the age pattern of child mortality. It is thus inappropriate to use proportions dead among children ever borne by women themselves born elsewhere to estimate either levels or (particularly) trends of child mortality. Information on duration of residence (or place of residence five years earlier) will again be helpful, however. Women aged 20 to 24 and resident five years or more will have borne virtually all their children in the area, and the same will apply to women resident 10 years or more aged 25 to 29. Thus, the proportions dead for these subgroups of women will be appropriate, but there will still be a

problem of fitting since the average parities may not be appropriate. Tabulation by duration of marriage, if available and if marriage represents the start of cohabitation, may be preferable to tabulation by age, since patterns of fertility by age are much more variable than those by duration of marriage.

Maternity histories

A maternity history is essentially a record of children ever borne and children dead with added information about dates of births and dates or ages of deaths. Thus, similar considerations apply to the use of maternity histories for estimating child mortality as to the use of proportions dead, with the exceptions that no fitting problems are involved and that much more detail is available. With little emigration, maternity histories for women born in the area reflect the mortality of the subgroup of children born to such women. With substantial emigration, the experience elsewhere of children who move out of the area will be included, but is unlikely to be important, and the experience in the area of children whose mothers move out will not be included, a loss that will only be important if there are substantial differences between the mortality in the area of such children and that of the children of reporting mothers. It is unlikely that much can be done to examine such differences since a maternity history survey that provides sufficient cases for an area-specific analysis is unlikely to be in national scope. Thus, the experience of women born in the area but living elsewhere will not be available for comparison.

If no additional migration information is available, the problem for immigrant women remains the same as that for proportions of children dead, namely, that it is not possible to distinguish the experience of children in the area from that of children outside the area. Here, however, a question on duration of residence has a profound effect, because it becomes possible for each child in the history to exclude all experience before the mother became a resident and to include only that experience during the mother's residence. This procedure assumes, of course, that the residence of the children is identical to that of the mother, but this assumption is reasonable for young children whose mortality rates are highest. Any maternity history survey of the area should include a question on duration of residence of each woman.

Survival of previous child

The survival of previous-child method was developed primarily to provide a way of estimating child mortality by adding a simple question to administrative data collection systems (birth notification, hospital or immunization records, for example). However, it has also been used in surveys, though it is not clear how suitable it is for such a purpose (survey information on the survival of each woman's most recent child has often been found to result in substantial underestimates of child mortality). However the data are collected, and assuming them to be reasonably accurate, the focus of this procedure on recent events to young children makes it particularly suitable for small area applications. Even substantial emigration of women born in the area will have little aggregate effect over a period of only three or four years unless child mortality differentials by

whether or not the mother migrated are very large. Even for women born elsewhere, it is only those cases where the previous child was also born elsewhere that will distort the measure; such cases will be few and their impact will also be small unless the mortality differentials between the area of birth and the area of study are very large. An additional question put to the mother on duration of residence, and limiting analysis to mothers resident five years or more, would effectively eliminate any distortion, except that introduced by eliminating from analysis the births in the area of recently arrived women.

This procedure thus seems well-suited to small area estimates of child mortality. It even essentially resolves a problem that remains if separate child mortality estimates are made for the children of native and non-native women, namely, how to weight the two estimates to arrive at a single measure for the area. A measure based on the survival of the previous child of all women, or of all women resident five or more years, should approximate general child mortality of children in that area. It should be noted, however, both that a single application provides no indication of trends, for which purpose continuous or periodic application is required, and that reporting accuracy still lacks widespread validation. Reporting completeness for previous children that have died may be better than that for most recent children that have died, because the dead child has been replaced, but extensive data validation is still required.

Adjusted registered deaths

Registered childhood deaths will only be valuable if they are available by residence in the area (or by place of registration, if such places can be regarded as approximately equivalent to residence in the area). The advantages of registered deaths are continuity, of importance to trend estimation, and that they reflect deaths in the area regardless of migration. Registration of infant deaths will rarely be adequately complete, but deaths at ages from one to four may be usable. Evaluation can be carried out for years for which the population aged one to four is known by calculating the age-specific mortality rate, finding the implied mortality rate in a system of model life tables and comparing it with estimates of child mortality level from other sources. If coverage appears close to complete, rates for other years can be calculated, obtaining denominators by interpolation between observed values. This approach relies heavily on potentially unreliable information, but it is relatively free of the problems introduced by studying sub-national populations. If the necessary data are available an attempt should be made to use them.

Evaluation and adjustment of registered deaths

The first strategy, of making the data conform to the assumptions of the methods, is of little use for evaluating and adjusting deaths after childhood. Stable or general methods for comparing deaths by age with population by age could theoretically be applied to deaths classified by area of birth (wherever they occur) and population by area of birth (wherever they now are) but such a breakdown of registered deaths is extremely unlikely to exist, and even if it were to exist any resulting estimate of registration completeness need have

little to do with registration completeness in the area of interest. Thus, we need to apply the second strategy, of using methods whose assumptions do not exclude open populations. There are two rather similar methods that can be applied if two census age distributions with intervening deaths by age are available.

The first method derives from the general age distribution equation of Preston and Coale (1982). This general equation is

$$N(a) = N(0)\ e^{-\int_0^\alpha r(x)dx} \qquad e^{-\sum_i \int_0^\alpha \mu_i(x)dx} \qquad (1)$$

where N(0) and N(a) are the populations of exact ages 0 and a respectively, r(x) is the growth rate at age x, and $\mu_i(x)$ is the force of attrition factor i at age x. If we consider only two types of attrition, $\mu_1(x)$ for mortality and $\mu_2(x)$ for emigration, and starting at age 5 instead of age 0, to avoid problems with childhood mortality, taking logs and rearranging equation 1 gives

$$\ln (N(a)/N(5)) + \int_5^\alpha r(x)dx = -\int_5^\alpha \mu_1(x)dx - \int_5^\alpha \mu_2(x)dx \qquad (2)$$

The left hand side of this equation can be obtained from the two census age distributions for the area, though the term in r(x) will be sensitive to any change in census coverage. Age specific mortality rates calculated from registered deaths and average intercensal exposure to risk can be used to represent the μ_1 term; if deaths are registered with a completeness C that is constant at all ages above 5, then

$$\int_5^\alpha \mu_1(x)dx \simeq (5/C) \sum_5^{\alpha-5} {}_5M_x$$

Net migrants will not normally be recorded directly, but background knowledge of the area may permit the selection of a model net migration schedule to provide rates for the μ_2 term. With a scaling factor g that is constant at all ages but not known,

$$\int_5^\alpha \mu_2(x)dx \simeq (5/g) \sum_5^{\alpha-5} {}_5E_x$$

where ${}_5E_x$ is the net emigration rate for the age group x, x x + 5. Substituting these values into (2), using a discrete approximation for $\int_5^\alpha r(x)dx$ and dividing through by $\sum_5^{\alpha-5} {}_5M_x$ gives

$$\frac{(1/5)\ln(N(a)/N(5)) + \sum_5^{\alpha-5} {}_5r_x}{\sum_5^{\alpha-5} {}_5M_x} = -\frac{1}{C} - \frac{1}{g}\,\frac{\sum_5^{\alpha-5} {}_5E_x}{\sum_5^{\alpha-5} {}_5M_x} \qquad (3)$$

A plot of the left hand term against the right hand ratio for different values of a should yield a straight line whose intercept is a function of C and whose slope is a function of g. Both the mortality and emigration patterns are evaluated, and can be adjusted for level if a good straight line is obtained.

The second method is rather similar, cumulating downwards instead of upwards. In any population, or part of a population, the growth rate is equal to the entry rate less the loss rate. Entries into the population aged a and over can occur either by births or birthdays, N(a), or by immigration at ages above a, I(a+), and losses can occur through deaths D(a+) or by emigration, E(a+). Thus in terms of rates,

$$r(a+) = \frac{N(a)}{N(a+)} - \frac{D(a+)}{N(a+)} + \frac{I(a+) - E(a+)}{N(a+)} \qquad (4)$$

Rearranging terms, replacing D(a+) by registered deaths D^o (a+) adjusted for their completeness C, and replacing net immigration by a set of model rates ${}_5NM_x$ adjusted for level by g and weighted by the population of each age group gives

$$\frac{N(a)}{N(a+)} - r(a+) = \frac{1}{C}\,\frac{D^o(a+)}{N(a+)} + \frac{1}{g}\,\frac{\sum_{\alpha}^{\omega} {}_5NM_x\ {}_5N_x}{N(a+)}$$

Dividing through by D^o (a+)/N(a+) then gives the desired straight line relationship,

$$\frac{N(a) - N(a+)\,r(a+)}{D^o(a+)} = -\frac{1}{C} + \frac{1}{g}\,\frac{\sum_{\alpha}^{\omega} {}_5NM_x\ {}_5N_x}{D^o(a+)} \qquad (5)$$

The parallel with equation 3 is obvious, one working with rates and the other with numbers. Though empirical tests have not been possible, both methods may be expected to give reasonable and similar results, as long as two conditions hold, because the age patterns of deaths and migrations are likely to be substantially different. The two conditions are that census coverage does not change and that the migration flow is predominantly in one direction. Note also that if registered deaths by age are not available, equation 5 should not be used with national registered deaths; rather, such deaths should be used to calculate age specific mortality rates, which can then be used to estimate deaths in the area as the product of the age specific rates and average intercensal populations of each age group.

Intercensal survival methods. The two methods based on equations 3 and 5 are essentially intercensal survival methods, though incorporating additional information. Using model schedules of mortality and migration, both can be applied to any pair of census age distributions for a given area. However, some progress may be possible without additional information by applying the first strategy, using conventional intercensal survival methods with an essentially closed population, in this case the population born in the area, wherever recorded by the two censuses. The measures will, of course, reflect some mortality outside the area, though unless emigration is heavy or mortality differentials very large the impact will be small, and will fail to reflect some mortality in the area, that of immigrants, though again the impact will be large only if the mortality differentials involved are large.

A more serious problem with intercensal survival techniques may often be change in enumeration completeness from one census to the next. Even quite small changes, of 1 or 2 per cent, can affect summary measures of expectation of life at specified ages by much larger relative amounts. With an open population, it is hard to detect such changes because age patterns of net migration and of coverage change may be similar. With a closed population such as that born in the area, however, it becomes possible to evaluate coverage change using the relationship

$$N^{o}(a)/N^{o}(a+) - r^{o}(a+) = (1/t)\ln(k/k) + (k_1 k)_{1/2/c}^{\frac{1}{2}}[D^{o}(a+)/N^{o}(a+)] \qquad (6)$$

where N^{o} (a) and N^{o} (a+) are the observed populations at and over age a respectively, r^{o} (a+) is the observed rate of growth of the population age a and over, t is the intercensal period in years, D^{o} (a+) is the observed deaths at age a or over, and k_1, k_2 and c are completeness factors, assumed constant by age, for the first and second censuses and for intercensal deaths respectively. The N^{o} and r^{o} terms are calculated from the two age distributions of people born in the area, and the D^{o} can be approximated by using model age-specific mortality rates or national registered deaths. A straight line can be fitted to points for successive ages to estimate the intercept, (1/t) log (k_1/k_2), and the slope, $(k_1 K_2)^{1/2/c}$. The estimated factors will refer to the population born in the area, rather than the population of the area, but again, so long as either the population born in the area but recorded elsewhere is not large or differentials in coverage and mortality between the non-migrant and out-migrant populations are not large, the factors k_1 and k_2 can be taken as approximations to the values for the area itself. The ratio k_1 to k_2 can then be used to adjust the census populations for coverage change, either for the population of the area, in which case open population methods would then be used, or for the population born in the area, in which case conventional intercensal survival methods would be used. A check of this sort on census coverage should always be made, however approximate the specification of the death rate term in equation 6.

Survival of close relatives. These methods are of limited usefulness for small area estimation because the basic proportions are averages of a wide range of survival probabilities often over lengthy periods. Although survival of first spouse is theoretically most suitable for small areas because the exposure periods are relatively recent and the range of survival ratios averaged is narrow, practice has shown the data to be of poor quality, and indicates that survival of mother may be the least unsuitable of these methods for subnational estimation. The first strategy can be used here, tabulating and analysing the data on survival of mother for the population born in the area. The resulting estimates will be valid, within the limits of the methodology, for at least a sub-group of the population as long as a large proportion of the mothers of such respondents still live in the area, or lived there until their death. There is no way to use these methods, to estimate mortality of immigrants, however. Even recent extensions of the maternal orphanhood approach, asking additional information about the date, year or broad period of the death (Chackiel and Orellana, 1985), do not increase the areal specificity of the information though permitting the use of shorter exposure periods.

Multi-round or continuous surveillance surveys. Surveys of this sort provide direct counts of both in and out migrants and, as such, require no estimation methodology. Indeed, the information obtained could be a useful basis for selecting, or even constructing, a model migration schedule. Equations 3 and 5 provide a basis for evaluating the events recorded against the changes in size and age composition of the area's population.

Intercensal residuals. Methods based on intercensal residuals to estimate migration are clearly designed for application to an open population, though the populations can usefully be defined in such a way as to concentrate either immigrants or out-migrants in order to increase events relative to population at risk. A formulation for obtaining the residuals that avoids the inconvenience of odd intercensal intervals and migrant mortality is as follows. 1/ If we consider an age group x, x + 5 enumerated at two censuses at times t1 and t2, the population of the group at t2, ${}_5Pt1_x$, will be equal to the population of the age group at t1, ${}_5Pt1_x$, plus all those having x^{th} birthdays during the period, E_x, less all those having $x+5^{th}$ birthdays, E_{x+5}, plus all the net immigrants aged x, x + 5, ${}_5I_x$, less all the deaths of people aged x, x + 5, ${}_5D_x$. Thus

$$sI\chi = 5P_{\chi}^{T2} - 5P_{\chi}^{T1} + E\chi+s - E\chi + sD\chi$$

If E_x and E_{x+5}, are estimated as the geometric means of the initial and final populations in the five year age groups above and below x and x + 5, respectively, multiplied by the intercensal interval, and divided by the width of the age group,

$$E\chi = (1/5)\ (t2 - t1)\ (sP_{\chi-s}^{T1}\ sP_{\chi}^{T1}\ sP_{\chi-s}^{T2}\ sP_{\chi}^{T2})_{1/4}$$

and

$$E\chi+s = (1/5)\ (t2 - t1)\ (sP_{\chi}^{T1}\ sP_{\chi+s}^{T1}\ sP_{\chi}^{T1}\ sP_{\chi+s}^{T1})_{1/4}$$

If ${}_5D_x$ is estimated from an age-specific mortality rate ${}_5M_x$ and person-years lived during the period, in turn estimated as the geometric mean of the initial and final population multiplied by the intercensal interval,

$$sD\chi = sM\chi\ (t2 - t1)\ (sP_{\chi}^{T1}\ sP_{\chi}^{T2})_{1/2}$$

Substituting these three approximations into the expression for net immigrants then gives

$$sI\chi = sP_{\chi}^{T2} - sP_{\chi}^{T1} + (t2-t1)\ (sP_{\chi}^{T1}\ sP_{\chi}^{T2})_{1/4}\ [(sP_{\chi+s}^{T1}\ sP_{\chi+s}^{T2})_{1/4} - (sP_{\chi-s}^{T1}\ sP_{\chi-s}^{T2})_{1/4} + (sP_{\chi}^{T1}\ sP_{\chi}^{T2})_{1/4}\ sM\chi]$$

1/ The author's thanks are due to John Wilmoth of the Office of Population Research, Princeton University for suggesting the applicability of this formulation to the estimation of migration.

This expression has the advantages over conventional representations that it is easy to apply regardless of the exact length of the intercensal interval, takes the mortality of migrants into account, and provides age-specific numbers of net migrants for periods rather than cohorts. The migrants still drop out as a residual, however, so this formulation is no less sensitive to changes in enumeration completeness or mis-specification of mortality conditions than the alternatives.

The first strategy is also applicable in this instance. If birthplace information is available, the procedure can be applied separately to a population of in-migrants, those born outside the area, and to a population of potential out-migrants, those born in the area. Both populations will, of course, experience moves in the other direction, as a result of return migration, but moves in the anticipated direction will predominate. Such disaggregation should prove methodologically revealing as well as providing largely independent information about the two flows that together make up net migration.

Analysis of birthplace and other migration data. The role of these data is to provide the information necessary for defining populations to which indirect methodologies can be applied. There are no methods applicable to such data per se, so no further discussion will be given.

Summary and conclusions

Indirect estimation techniques for demographic parameters generally assume a closed population and sometimes even a stable one. Populations of small areas, particularly those affected by major development projects, are unlikely to meet this assumption. There are two obvious solutions, one to use existing methods, but to apply them to populations that can be regarded as approximately closed to migration, and the other to use more flexible methods designed to handle open populations. The first approach is generally to be preferred, in some cases merely because observed differentials between migrant and non-migrant populations may be of interest, but in others because there is no alternative. Information on birthplace and duration of residence or residence at some specified earlier time will be essential for implementing this approach. Further, some indirect methods which strictly assume a closed population may perform reasonably well even when applied to an open population. Use of the first approach can be expected to produce reasonable estimates of fertility (especially from the own-children method, though P/F and C/B ratios are likely to be revealing) and child mortality (especially from maternity histories, proportions dead amongst children ever borne, and survival of previous child). The methods can be applied to a single set of data, particularly from a census that permits identification of the project area both for the current population and for birthplace or residence at some earlier time. Applications to a series of consistently defined data sets will prove of great value, however, for evaluating the data to distinguish errors from genuine patterns. The second approach is clearly essential for the study of migration and is generally preferable for the study of adult mortality, though elements of the first approach can also be incorporated. A series of consistently defined data sets, including at least two census age distributions, is required for this approach; specific information about migration will be useful, but is not required.

The nature of the available methods has strong implications for the design of any ad hoc survey that may be planned. Multiple consistent data sources are a high priority, so the survey should be designed to complement existing data sources, for example using a study area that can be readily identified in an earlier census and repeating the questions and definitions used earlier. Before any detailed planning is undertaken, therefore, all existing data sources should be reviewed and their potential for providing the type of information needed for small area estimation assessed. Second, any survey must include information on migration, either through continuous recording or through the inclusion in a single round retrospective survey of questions on birthplace, duration of residence, and place of residence five years earlier. Third, the survey should, if possible, incorporate a maternity history. Fourth, though a multi-round or continuous surveillance survey is desirable, the costs of such a survey, and particularly the small sample size imposed by the costs, should be weighted against the analytical benefits, bearing in mind that many events of interest are relatively rare. Fifth, consideration should be given to the relatively inexpensive data collection strategy of modifying an existing administrative collection system by including a question on the survival of the previous child.

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Annex

A PROCEDURE TO EVALUATE QUINQUENNIAL CENSUS DATA, INCLUDING RESIDENCE FIVE YEARS EARLIER

Suppose a certain country has two census counts, separated by five years and tabulated by province, and that the second census included a question on province of residence five years earlier. If emigration from the country is negligible, the population at census 1 resident in province A can be projected five years to estimate the total population at census 2 reporting residence in A five years earlier (the mortality schedule used in the reverse projection will not be crucial).

Similar comparisons can be made for all provinces. For any province i, the population recorded by census 2, as resident in i five years earlier, N_i^R will differ from the projected estimate, N_i^E as a result of reporting errors. One such error is likely to be the under-reporting of residence elsewhere, that is, a tendency to report residence five years earlier as the province of current residence even if actual residence was in some other province. This error will result in N_i^E values being smaller than N_i^R values for provinces of net immigration, and N_i^E exceeding N_i^R for provinces of net emigration.

Both N_i^E and N_i^R can be seen as having two components: those non-migrants still living in province i at the time of census 2, NM_i^E and NM_i^R, and those migrants living in some other province at the time of census 2, M_i^E and M_i^R. The terms superscripted by E represent true values based on actual residence at census 1 but only N_i^E, the sum of NM_i^E, and M_i^E, is known. The terms superscripted by R represent recorded values from census 2, and both NM_i^R and M_i^R are known but are assumed to be affected by reporting error.

Let us now assume that the propensity to misreport true residence in some other province five years earlier as unchanged residence in the same province is a constant k across the whole population of movers, and the propensity to misreport unchanged residence as changed residence is zero. With this assumption, we can relate the E and R terms. Specifically, the true number of movers, M_i^E, will be equal to the reported number of movers, M_i^R, divided by (1-k):

$$M_i^E = M_i^R / (1-k)$$

Similarly, the true number of non-movers, NM_i^E, will be equal to the reported number of non-movers, NM_i^R, less the number of misreporting true movers. The number of true movers into province i, I_i^E, is equal to population of province i at census 2, P_i, less the population of non-movers, NM_i^E. The number of true movers into i misreporting as non-movers is kI_i^E, so

$$NM_i^E = NM_i^R - kI_i^E = NM_i^R - k(P_i - NM_i^E)$$

Simplifying,

$$NM_i^E = (NM_i^R - kP_i)/(1-k)$$

Since N_i^E is equal to the sum of NM_i^E and M_i^E,

$$N_i^E = M_i^R/(1-k) + (NM_i^R - kP_i)/(1-k)$$

Rearranging for k then gives

$$k = (M_i^R + NM_i^R - N_i^E)/(P_i - N_i^E) \qquad (A.1)$$

All the terms in equation A.1 are known from the second census (M_i^R, NM_i^R and P_i) or estimated from the first census (N_i^E). Thus an estimate of R can be arrived at for each province, though on the assumption that k is constant across provinces. Thus a final estimate could be obtained by averaging the individual estimates of k. Note that estimates k will be unstable if P_i and N_i^E are similar, that is, if net migration for area i is small. Unstable values could be excluded from the averaging process, or a weighted average could be found, using absolute values of $P_i - N_i^E$ as weights.

Only one simplified type of error has been considered. Change in census coverage will have a large effect on the results, although, with sufficient provinces and wide variations in $P_i - N_i^E$ values, there is some prospect of estimating relative census coverage. Suppose that the first census was proportion j complete relative to the second census, constant for all ages and provinces. Then N_i^E based on the first census can be made consistent with the coverage of the second census by dividing it by j, so equation A.1 then becomes

$$k = (M_i^R - NM_i^R - N_i^E/j)/(P_i - N_i^E/j)$$

Where N_i^E is now the projected population recorded by census 1 as resident in province i.

Rearranging terms then gives

$$(M_i^R + NM_i^R)/N_i^E = (1-k)/j + kP_i/N_i^E \qquad (A.2)$$

This equation shows a straight line relationship between $(M_i^R + NM_i^R) / N_i^E$ and P_i/N_i^E, the slope of the line being k and its intercept (1-k)/j. Thus, plotting observed ratios for all provinces i may, if a straight line relationship is evident, provide estimates both of k and j.

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